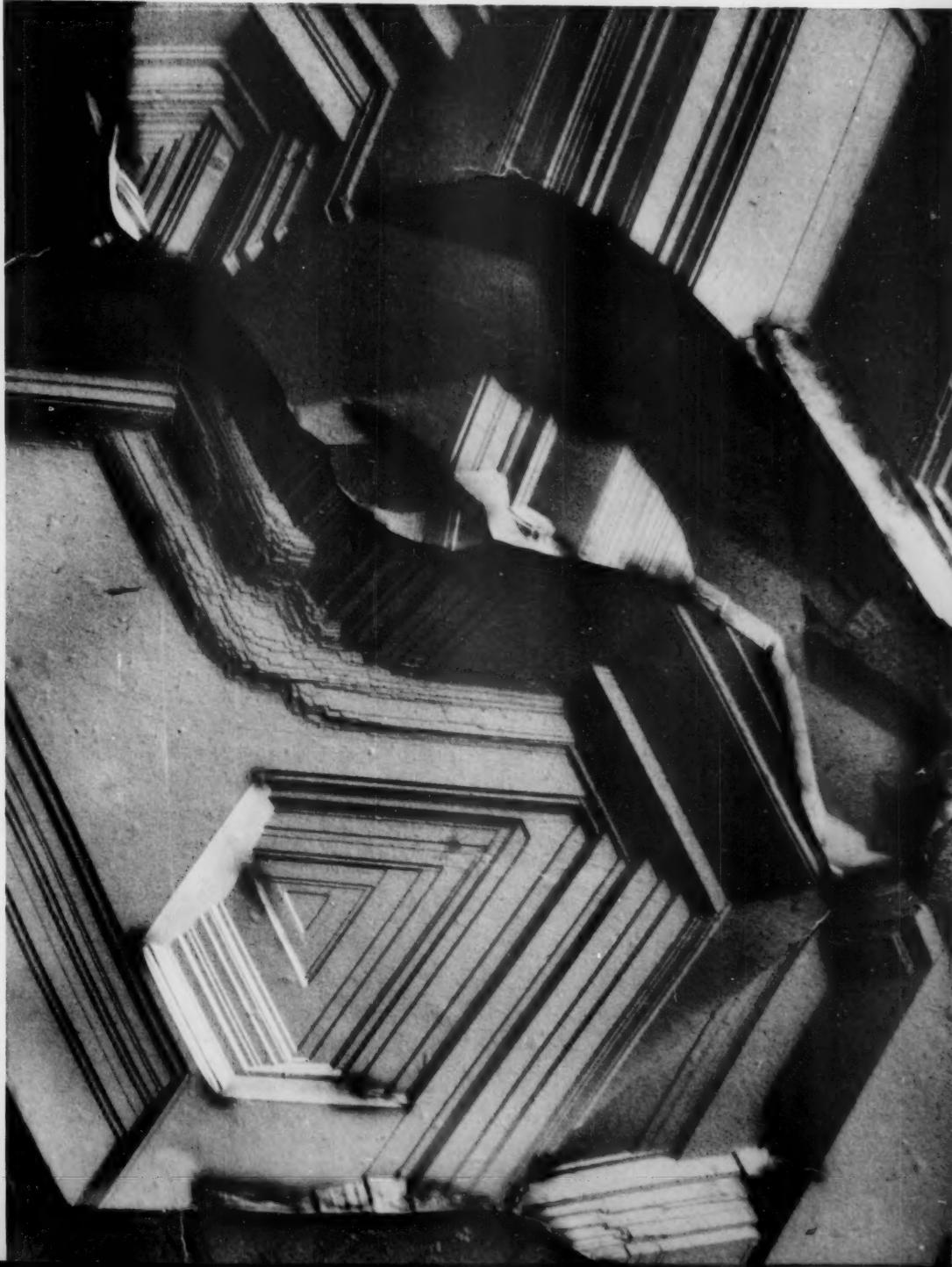


SCIENCE

8 September 1961

Vol. 134, No. 3480

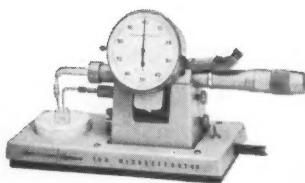


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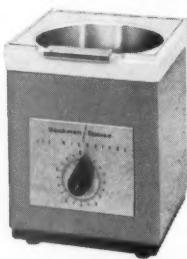
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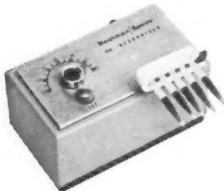
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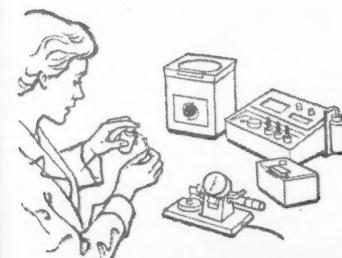
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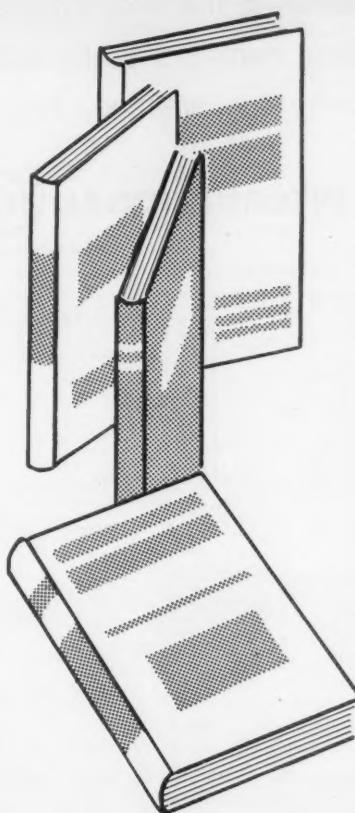
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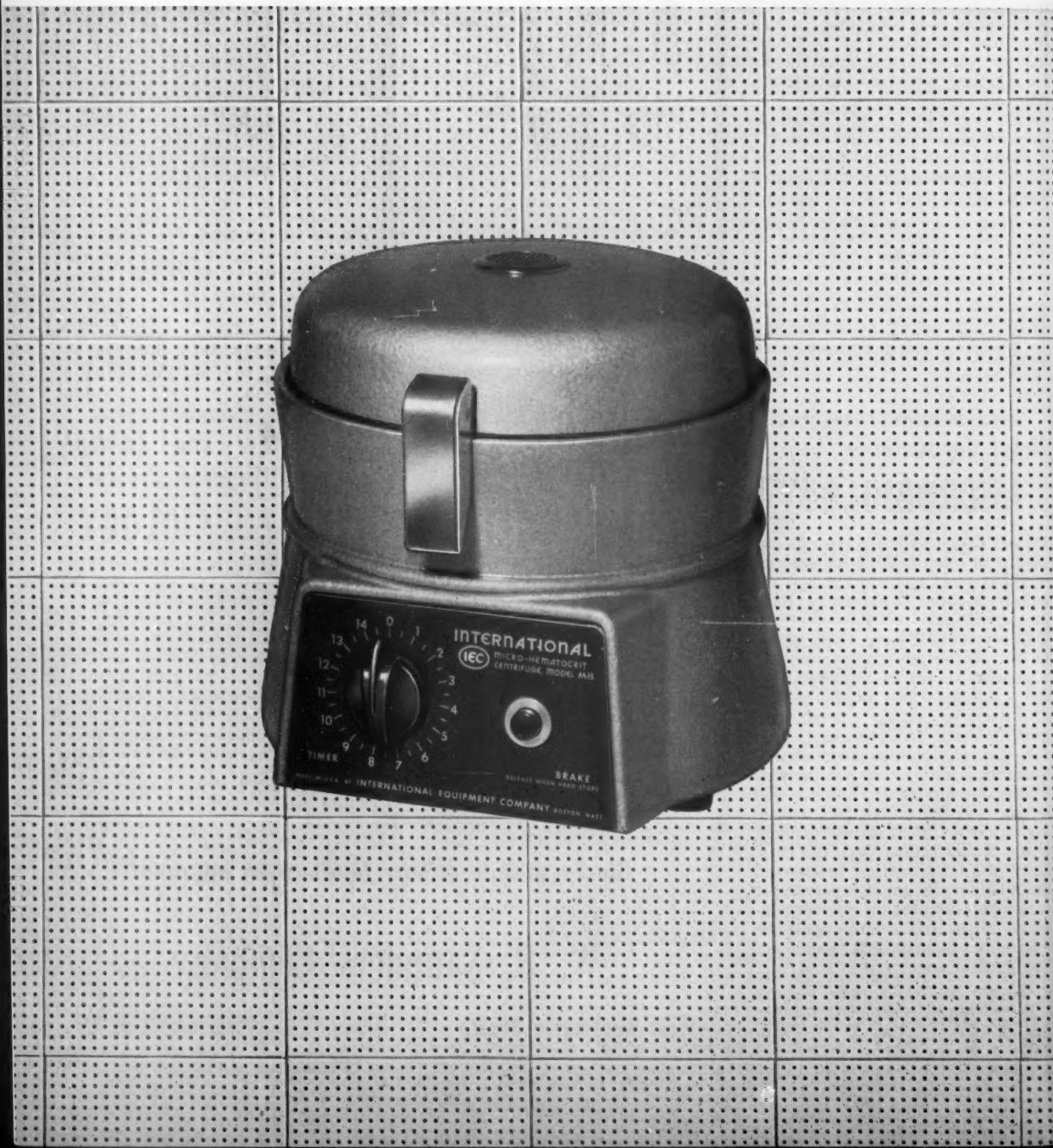
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Editorial	Two Cheers for West Ford	641
Articles	Human Evolution by Voluntary Choice of Germ Plasm: <i>H. J. Muller</i> This procedure should be more acceptable and effective than differential control over family size.	643
	Optical Rotatory Dispersion: <i>C. Djerassi</i> Investigation of the phenomenon 140 years after its discovery sheds light on problems of organic chemistry.	649
Science and the News	End of the Test Ban: The White House Response Is Restrained; Educational TV in Court	655
Book Reviews	R. W. Clark's <i>The Birth of the Bomb</i> , reviewed by <i>M. C. Henderson</i> ; other reviews	661
Reports	Influence of Soil Oxygen Concentrations on the Development of <i>Meloidogyne javanica</i> : <i>S. D. Van Gundy</i> and <i>L. H. Stolzy</i>	665
	Simultaneous Appearance of Free Arginine and Deoxyribosidic Compounds during Mitosis: <i>M. Nasatir, A. M. Bryan, A. Rake</i>	666
	Virus-Tumor Synergism: <i>V. Riley</i>	666
	Transreplication and Crossing Over in <i>Sordaria fimicola</i> : <i>Y. Kitani, L. S. Olive, A. S. El-Ani</i>	668
	Stimulation of Uterine Contraction by Extracts of the Cockroach, <i>Periplaneta</i> : <i>L. Barton Browne, E. S. Hodgson, J. K. Kiraly</i>	669
	Succession in Desert Vegetation on Streets of a Nevada Ghost Town: <i>P. V. Wells</i>	670
	Biosynthesis of Stigmasterol in Tomato Fruits: <i>R. D. Bennett</i> et al.	671
	Chemical Analysis of Surfaces by Use of Large-Angle Scattering of Heavy Charged Particles: <i>A. Turkevich</i>	672
	10-Methoxyharmalan, a Potent Serotonin Antagonist Which Affects Conditioned Behavior: <i>W. M. McIsaac, P. A. Khairallah, I. H. Page</i>	674
	Eyestalk Movements Induced by Polarized Light in the Ghost Crab, <i>Ocypode quadrata</i> : <i>Hermann Schöne</i> and <i>Hedwig Schöne</i>	675
	General Method of Plotting Kinetic Data for Reactions of Any Order: <i>H. Pessen</i>	676
Departments	Carbon Conference; Forthcoming Events; New Products	678
	Letters from <i>J. W. Hinkley, L. Fleming, R. H. Linnell, D. W. Robertson, F. Machlup, L. Pap</i>	637
Cover	Replicated surface of a stainless steel coupon or platelet oxidized for 32 hours at 1500°F and annealed for 500 hours at 1500°F in carbon dioxide. Electron microscope, $\times 25,000$. [T. E. Willmarth, Analytical Chemistry Division, Oak Ridge National Laboratory]	

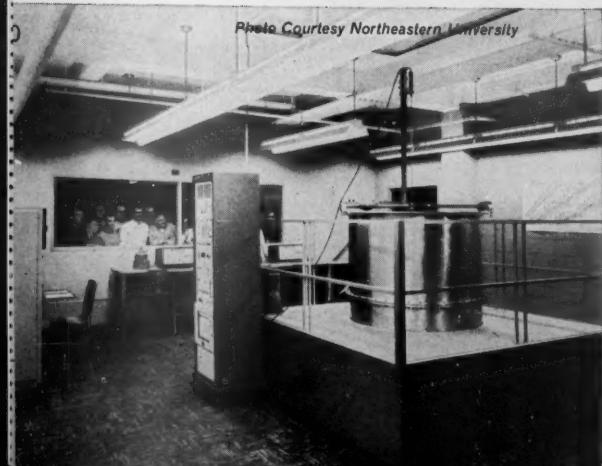
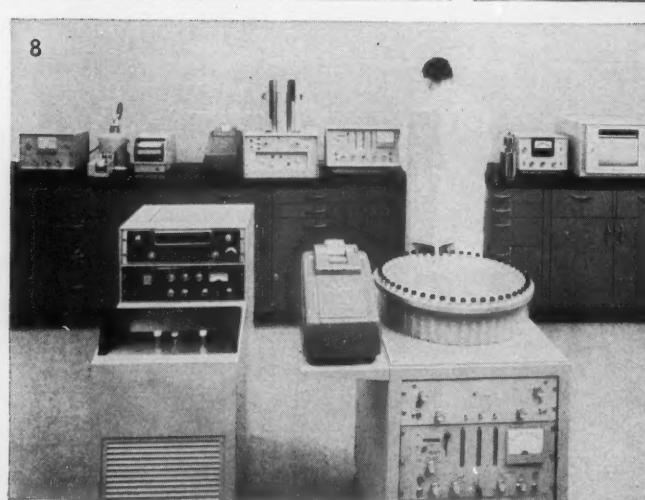
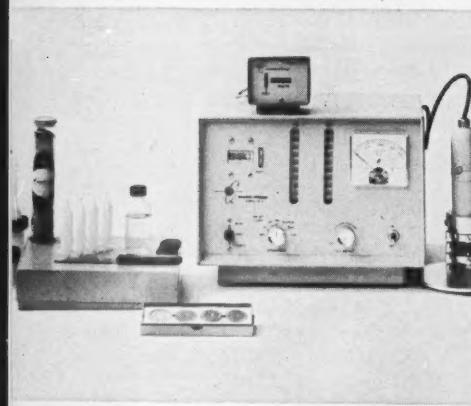
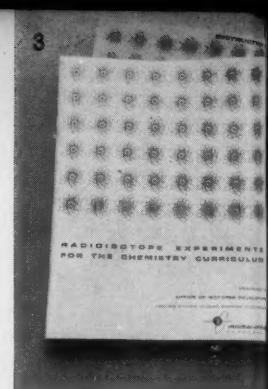
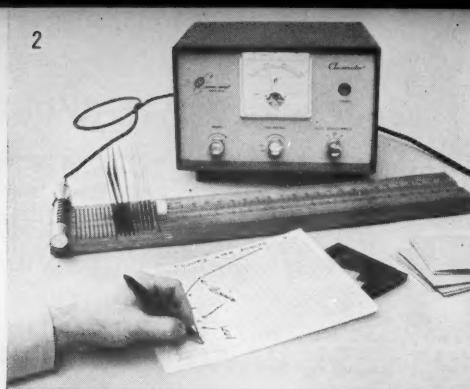
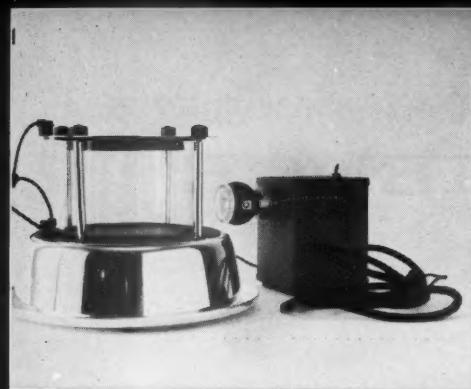


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How to bring radioisotopes into the science curriculum

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RADIOISOTOPE procedures offer students penetrating insights into the laws of nature. Forward-looking educators are using the equipment illustrated at the left in lectures, for student experiments, and in research. These eleven photographs suggest the scope and completeness of Nuclear-Chicago's instrument line for teaching and research. Systems range from low-cost equipment for demonstration to sophisticated instrumentation for fundamental investigation. All are characterized by reliability, precision, versatility, and inherent safety.

Schools now employing radioisotope techniques will want to consider program expansion to take advantage of the newer products illustrated. Others may well regard the apparatus presented here as a planning guide.

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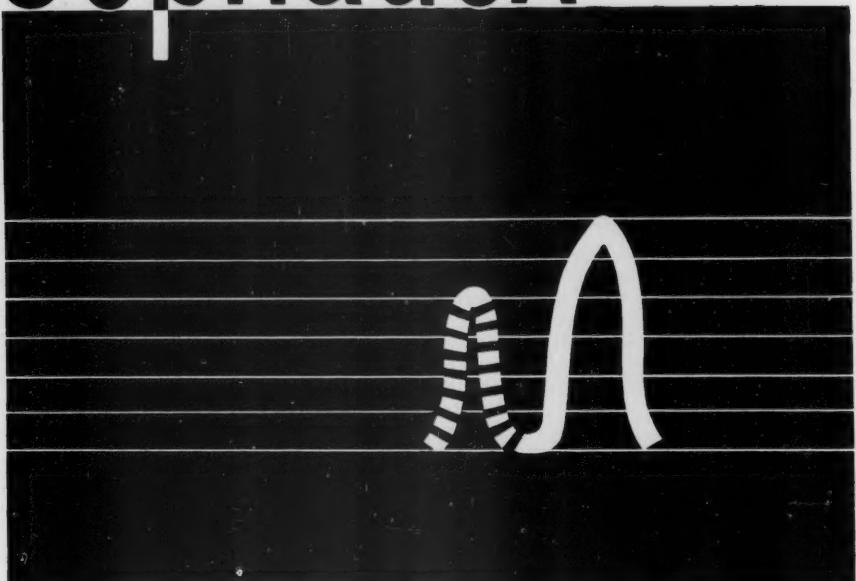
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Letters

Patent System

I would like to comment on the article "Patents and inventive effort" [Science 133, 1463 (1961)]. First, may I say I was impressed by the unscholarly character of the article. While Machlup's analysis of the statistical details set forth impressively in the tables was straightforward, the phraseology and tone employed in the first several paragraphs and in the concluding paragraph indicated the author's bias rather than scholarly objectivity.

My specific comment concerns his references to the "faithful," in which category I fall, though I do not admit to the "faith alone" derogation of the author. Machlup, in common with many of his contemporaries and predecessors, seems to have the concept that the patent system is a force in our highly complex economic society that can be isolated from all other forces and measured. He and others have spent much effort in attempting to do this, and have rushed into print when they have developed sets of statistics and discovered correlations which may be adverse to the United States patent system. They pounce upon these and draw conclusions without penetrating more deeply to see if a more refined and thoughtful analysis of some of the data could tell why the correlations, or lack thereof, exist. Rather, they hurriedly publish papers based on inference and conjecture, which soon are accepted as authoritative documents and conclusive studies. Machlup's sole factual contribution in his paper is a tabular presentation of information well known and widely discussed heretofore.

The "faithful" base their judgment on many years of experience of intimate working with the patent system. From this they have derived knowledge of how the patent system itself is integrated into, and operates within, the

very complex larger economic system that governs the advancement of technological industry in the United States. They recognize that it is only one factor in this development, sometimes playing a major, and sometimes a minor, role. It may be impossible to isolate the patent system from this highly complicated free enterprise system, and prove conclusively by a series of tables that it is indeed important. Nevertheless, knowledge of the part that it plays has convinced them that patents are a real factor in our overall economy. It is curious that so many students of our patent system seem unable to comprehend simple facts about our economic system.

J. W. HINKLEY

Research Corporation,
405 Lexington Avenue,
New York, New York

The article by Fritz Machlup requires comment.

Machlup's argument appears to be this: Small industrial firms have said that patents are useful, but large firms whose competition is "oligopolistic" say that patents are not useful; therefore the American patent system should be abolished.

If this conclusion be true, then patents must not be useful to society; and if we accept this, it must follow that the word of the "oligopolies" should be given weight over that of the smaller firms and individuals in deciding whether the patent system should be retained.

The stationarity of the rate of patenting might be explained by a rise in oligopolism. But an equally good explanation would be what Roberts has called "the persistent weakening of the patent system by court decisions since 1930" [Walter Van B. Roberts, *Inventors and Inventions* (McGraw-Hill, New York, 1957)].

There is no evidence that oligopolistic

industrial research has produced more than such work in the smaller organizations which Machlup admits do advocate patents. The reverse appears to be true. The automobile and the electrical power equipment industries, for example, have produced little useful novelty for two or more decades.

Much non-oligopolistic competition prevails in the more technical industries, and it seems reasonable that their expressions of favor toward patents are based on experience at least as much as "faith."

Machlup must mean that (i) all industry is oligopolistic, or soon will be, or (ii) only oligopolistic industries should be considered in regard to patent policy. If the first is true, he should say so; if the second, he should explain why.

LAWRENCE FLEMING
1550 Old House Road,
Pasadena, California

I feel it is unfortunate that the article, "Patents and inventive effort," was published as it was. Machlup leaves the definite impression that he does not favor the present patent system, although he doesn't seem to have quite the courage to state his feelings so bluntly. But the really important fault in the article is the lack of any discussion of the crucial point of secrecy versus publication via a patent. A most basic and important concept in the establishment of the patent system is that the inventor obtains exclusive rights for a limited time in exchange for his publication of his invention. This publication stimulates others to new and improved inventions and thus is greatly to the general public interest. Literally thousands of inventions and contributions to the development of civilization have been stimulated by published patents. Without some patent protection, secrecy would certainly be much more widespread and it seems certain that such secrecy would greatly slow down technological progress. This important point should be understood by Machlup and *Science* readers.

ROBERT H. LINNELL
University of Vermont, Burlington

The Machlup article is one of the most interesting probes of the patent system that has come to my attention, and I believe it follows a fairly clear path toward the truth—as far as the author goes. I would agree on points that could be restated: (i) that it is

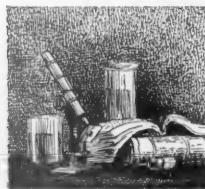
IT HAPPENED THIS MONTH...

a glance at yesterday in relation to today



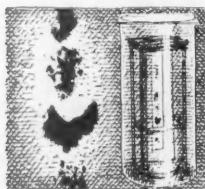
IN SEPTEMBER — (1923) — two biochemists at Toronto challenge current ideas about the amino acid composition of hemoglobin. Three years ago, Furth and Lieben — using the Voisinet reaction — had concluded that hemoglobin is free of tryptophane. Now, Hunter and Borsook¹, using the method of Folin and Looney, present data which suggests that the globin component contains 2 tryptophane residues. Further analysis indicates also the presence of 4 residues each of tyrosine and arginine, 8 of histidine, 10 of lysine, and 100 of other amino acids, including dicarboxylic acids.

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IN SEPTEMBER — (1908) — there is abstracted² a report by Büchner and Klatte on some attempts to determine the nature of the coenzyme of yeast press juice. Upon standing, press juice gradually loses its activity, and this is attributed to disappearance of the coenzyme. Activity can be restored by addition of boiled press juice. Lipase emulsions cause the boiled juice to lose its restorative powers, but proteolytic enzymes have no effect. Apparently the coenzyme contains an organic phosphoric ester which is split by the lipase.

Today the yeast press juice coenzyme is well known to the scientific world under such names as Coenzyme I, diphosphopyridine nucleotide, cozymase, and DPN. Schwarz BioResearch is also well known to the scientific world as a source of DPN and other metabolic cofactors such as ATP, ADP, adenylic acid, flavin adenine dinucleotide, guanosine diphosphate and guanosine triphosphate.



IN SEPTEMBER — (1954) — a note from Oak Ridge describes an ion-exchange method for separating 5' ribo- and deoxyribonucleotides. Since the deoxy-compounds are not separated from their ribose analogs by chromatography with simple acids or salts, the authors employed a borate complex method previously used to separate isomeric ribose phosphates. This technique is of special interest in view of recent evidence suggesting that both types of nucleotides may exist in the free state in body tissues.³

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1. Hunter, A., and Borsook, H.: Nitrogen distribution in globin. *J. Biol. Chem.* 57:507 (Sept.) 1923.
2. Büchner, E., and Klatte, F.: The coenzyme of yeast press juice. *Chem. Abstracts* 2:2568 (Sept. 19) 1908.
3. Khyn, J. X., and Cohn, W. E.: The ion-exchange separation of the 5' ribonucleotides and deoxyribonucleotides. *Biochem. et Biophys. Acta* 15:139 (Sept.) 1954.



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easier to demonstrate the importance of patents as lures for venture capital than as incentives to inventive effort, and (ii) that, by and large, patents assume more importance to individuals and small corporations than they do to large corporations.

Certainly large corporations are going to continue their research, patent system or no. But the point of perspective which I find lacking in this article is the significance of patents to the founding of entirely new industries which are starting from scratch and have no research organization or anything else. Perhaps the classic example of this is the aluminum industry, which could not have come into being when it did without the Pittsburgh bankers, who would not have put up the enormous initial outlay required without patent insurance to protect the money. Of course the aluminum industry has a rather peculiar aspect to its beginnings, because it had to start big—couldn't start small. This was because of the enormous electrical power requirements to operate reduction pots. But I would think that the system which did give us the aluminum industry and may some day give us another new industry of equal significance has paid its way for all time.

So what remains to be said after one reads Machlup is: True, one cannot demonstrate the need for the patent system to keep research laboratories of existing industries working and inventing, but the system can very well be the *sine qua non* of industries and enterprises outside the scope of activity of existing research facilities, government and private.

DONALD W. ROBERTSON

Robertson & Smythe,
155 East 44 Street,
New York, New York

Hinkley, who counts himself among the "faithful," believes that I am biased, apparently because I stated that no evidence has been found one way or another concerning the effects of patents upon inventive activity. If this is "bias," I must plead guilty; but I still hold that a scholar should search for evidence. Hinkley is clearly wrong when he says that I "rush into print" when I "have developed sets of statistics and discovered correlations which may be adverse to the United States patent system." One of the main points of my article was to show that certain statistical relations which others had developed did not, as they had thought,

demonstrate the absence of a positive effect of the patent system upon inventive effort.

Fleming jumps to the conclusion that I want to see the American patent system abolished. Linnell believes that I lack the courage to state that I do "not favor" the present patent system. In my monograph (*Study No. 15, Subcommittee on Patents, Senate Committee on the Judiciary*) I stated explicitly that "it would be irresponsible, on the basis of our present knowledge [of the economics of patent protection], to recommend abolishing" the United States patent system. Nowhere in my article did I make a value judgment either for or against the patent system. Linnell is right in that I did not discuss in my article the question of "secrecy and disclosure." There are also many other important issues which I did not cover in that brief article. A discussion of the secrecy issue can be found in my monograph.

Robertson points to the role of patents in the emergence of new industries, such as the aluminum industry. This is an important matter, because it suggests that the effects of the patent system upon inventive activity in existing industries are not really relevant in an evaluation of the system as a whole. More thought will have to be given to the questions (i) what evidence there is to support the hypothesis that new industries could not emerge without the aid of patent protection, and (ii) whether certain changes in the system could be revised to make it even more effective in promoting entirely new industries. To ask these questions is not to suggest that the answers might more likely be one way than another.

One final point. Hinkley believes that "many years of experience of intimate working with the patent system" can help the practitioner to understand its effects upon the operation of the economy as a whole. This I must deny. Perhaps I may illustrate this denial by reference to other areas. No amount of experience in banking can give the banker an understanding of the effects of credit policies upon the economy as a whole. Not even the longest career in trade-union affairs and collective bargaining can teach the union secretary how union activity affects average real income of the total labor force. Neither the importer nor the business man competing with imports can learn from experience what effects tariffs may have upon real national income. If practical experience

were sufficient to teach us what we want to know, science would be superfluous. Needless to say, practitioners may also be scholars and researchers, and their achievements in these endeavors will be recognized as such to the extent that their findings are based on good evidence.

FRITZ MACHLUP

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Econology and Word Coinage

As a linguist, I am horrified at the coinage *econology*, proposed by John L. Kennedy in his review of Siegel and Fouraker: *Bargaining and Group Decision Making* (*Science*, 14 July 1961, p. 95), to designate a border discipline between economics and psychology. If we must invent new terms at the drop of a hat, why not do so with proper regard to the structure and "spirit" of the English language? Since *-ology* (but not *-nology*) is a well-established English suffix, anyone attempting to interpret this neologism is bound to analyze it as *econ-* plus *-ology*. But what is *econ-*? Granted that this form would make one think readily of economics, as it is intended to; however, by the same token it would mislead one into falsely analyzing *economics* as *econ-* plus *-omics*. Such an analysis would, of course, fly in the face of etymological facts, *economics* having been constructed out of Greek *oikos* "household" and *-nomos* "arranging, managing." More important, this would introduce confusion into the future handling of another well-established English "ending," namely, *-nomy* (and *-nomic*), as in *agronomy*, *taxonomy*, giving rise to a competing *-omy* (and *-omic*). Heaven forbid, the chain reaction thus started might even produce a faulty fission of *atomic* into *at-* plus *-omic*, causing havoc with the negative prefix *a-*.

I think I have a better suggestion, if a new label for certain interdisciplinary endeavors of economists and psychologists seems desirable: *psycho-economics*. This coinage would be more self-explanatory than *econology*, and it would be solidly patterned after such recent "hybrid" formations (already well entrenched in current scientific discourse) as *psycholinguistics*, *psychobiology*, and similar ones.

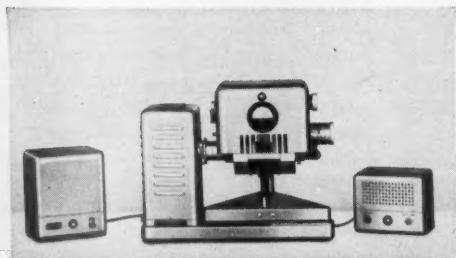
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Two Cheers for West Ford

At its recent assembly in Berkeley, California, attended by some 1000 astronomers from all over the world, the International Astronomical Union passed a resolution urging openness of procedure and great caution in the conduct of experiments that might affect the observations of optical and radio astronomers. This was quite right, but there was a second resolution opposing immediate carrying out by the United States of its project West Ford, a plan to place temporarily in orbit about 75 pounds of fine copper filaments to serve as a reflector of radio signals. The second resolution offers, in passing, one cheer for this country's behavior in the matter, but, so it seems to us, the United States deserves two cheers at least.

The International Astronomical Union was at pains to make clear just how any government should proceed when conducting an experiment that, in effect, uses the earth as a laboratory. It states that the government should make public its plans and the calculations justifying those plans, so that all interested scientists will be in a position to evaluate the program. The government should also invite interested scientists throughout the world to participate in making observations. But this is very much what the United States has done. Articles by American scientists published in the April issue of *The Astronomical Journal* describe the project and invite the cooperation of interested scientists.

The union is also concerned that no further launchings of reflecting belts be undertaken until the effects of the first experiment have been analyzed by project scientists in cooperation with other scientists. But again the United States had announced plans along the lines subsequently recommended by the union. In stating these requirements, the union acknowledges the actions of the United States, and this constitutes the one cheer offered.

There seem to be no difficulties about the effects of West Ford on the kinds of astronomical observations now possible. Opposition by the union arises out of fear that the belt might remain indefinitely in orbit, and so interfere, at some future date, with methods of astronomical observation yet to be developed. West Ford scientists intend, however, to orbit the belt in such a way that in a year or two solar radiation pressure will bring it into the atmosphere where it will be destroyed. The union wants more public discussion of this matter, and it is here that the United States could be in a better position. The calculations on which the contention of short life is based, although involving nothing particularly abstruse or secret, have not been published.

Failure to publish this material is an oversight that can easily be corrected, and there is reason to believe that this is soon to be done. In evaluating American conduct, however, the basic point to remember is that if our policy had been to run the entire experiment secretly, detection of the belt, according to informed sources, would have been unlikely in the extreme. Lacking advance notice of the project, no one would have been the wiser. The present American approach contrasts favorably with our handling of project Argus, in which, in 1958, without public notice, several atom bombs were exploded at low altitudes to create temporary radiation belts. It also contrasts favorably with the general Soviet practice of announcing launchings as accomplished facts.

For comments on another kind of experimentation that has worldwide effects, it will be interesting to see, at the forthcoming Pugwash-type conference in Colorado on nuclear weapons control, what Russian scientists have to say to their American colleagues.—J.T.

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Human Evolution by Voluntary Choice of Germ Plasm

This procedure should be more acceptable and effective than differential control over family size.

H. J. Muller

For some decades the term *eugenics* has been in such disrepute, as a result of its spurious use in support of the atrocities committed by those with class and race prejudices, that few responsible students of evolution or genetics have dared to contaminate themselves by mentioning it, much less by dealing with the subject except in condemnation. However, it is now high time to take new stock of the situation. For the odious perversions of the subject should not blind us longer to a set of hard truths, and of genuine ethical values concerning human evolution, that cannot be permanently ignored or denied without ultimate disaster. On the other hand, if these truths are duly recognized and given expression in suitable policies, they may open the way to an immeasurable extension and enhancement of the potentialities of human existence.

In view of the signal defeat in World War II of the leading exponents of racism—a defeat which is still gathering momentum—and the declining prestige afforded in the Western world to the claims of aristocratic or bourgeois class differentiations, it at last becomes feasible to return, in a more reasonable spirit, to the theme of prospective human biological evolution. Moreover, for this job of re-examination we are now provided not only with a better understanding of

genetic and evolutionary principles but also with a considerably reformed structure in most Western societies, liberalized mores, a heightened freedom of discussion, and a marked improvement in technologies, all of which combine to make possible approaches that earlier would have seemed out of the question.

It was Darwin who pointed out that modern culture is causing a relaxation and perhaps even a reversal of selection for socially desirable traits, and he expressed himself rather pessimistically about the matter, although in his time this process must have been much less pronounced than it is nowadays. His cousin Galton, impressed by Darwin's arguments concerning evolution in general as well as by those pertaining to man, but unwilling to accept defeat or frustration for humanity on this score, proposed the idea that the trend might be counteracted consciously. For this course of action he coined the term *eugenics*, included within which he understood all measures calculated to affect the hereditary constitution in a favorable way. As he pointed out, these measures might be of very diverse kinds, lying not only in such fields as medicine but also in education, economics, public policy in general, and social customs, although he did not contemplate drastic changes from the mores of that Victorian age.

Unfortunately, although Galton realized to some extent the influence of the social and familial environment in the shaping of people's psychological traits, he was not sufficiently aware of the profundity of the environmental control. He therefore made the naive mistake, so widespread in his day, of looking upon the performances of different ethnic, national and social groups as indicative of their genetic capabilities and inclinations, although there were plenty of object lessons of the comparatively rapid transference of cultures that should have taught him better. Later, it was the madness of such out-and-out racists and so-called "social Darwinists" as Madison Grant, Lothrop Stoddard, Eugen Fischer, Lenz (1), and the Hitlerites which, carrying these prejudices much further, brought such odium upon the whole concept of eugenics as to run it into the ground.

Meanwhile, a large group of psychologists, represented by the Watson school, and of other social scientists, social reformers, socialists, and communists, all of them persons of egalitarian sympathies, impressed by the enormous potency of educational and other cultural influences, and regarding all eugenics as a dangerous kind of reaction that threatened their own roads to progress, popularized the idea that differences in human faculties are of negligible consequence not only as between different peoples and social classes but even as between individuals of the same group. They held that genetics in man could be allowed to take care of itself. And even where some genetic defects were admitted to exist, it was maintained that improved medical, psychological, and other cultural ministrations would provide sufficient remedies for them. Moreover, added the many Lamarckians among these groups, the improvements thereby acquired would eventually pass into the hereditary constitution. In this way, not only would all men become equalized but they would rise to ever higher

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biological as well as cultural levels. By about 1936 it had become a dire heresy among the official communists to dispute this line of argument, and the word *eugenics* had become a favorite symbol of all that is vile.

It is no wonder that earnest students of evolution and genetics, confronted by the mighty currents of these contending movements, which had advanced into the area of power politics, seeing how intertwined truth and error had become, and aware that their own views would almost certainly be misconstrued, tended to withdraw into their ivory towers and to refuse to discuss seriously the possible applications of genetics to man. It is to their credit, however, that only a few floated with the current that happened to be around them. But even fewer tried to contend with that current by raising the voice of reason, for that way lay the path to martyrdom.

Perhaps the last attempt made, up until the past few months, to present an appraisal of eugenics undistorted by extremist politics was the drafting of the "Geneticists' Manifesto" (2) of 1939, signed by about a score of participants at the International Genetics Congress held in Edinburgh just as the curtain began to rise on World War II. In this document it was pointed out that by far the greatest causes of differences between human groups in regard to psychological traits were environmental, predominantly cultural, whereas in the causation of such differences between individuals within the same group, both environmental and genetic factors were very powerful, and often comparable in their potency of action. The need for far-reaching reforms—for affording more nearly equal opportunities to all groups as well as to all individuals and for removing biases—was stressed in this document, not only for the sake of persons directly concerned but also to provide a groundwork for the truer assessment of genetic differences, in the interest of more soundly based eugenics.

These reforms in society were also needed, it was pointed out, for the attainment by the population of a sounder set of values, applicable equally to eugenic and to cultural purposes: values by which active service and creativity would be regarded more highly than either passive submissiveness or self-aggrandizement. The "Geneticists' Manifesto," far from discarding the concept of eugenics per-

se, acknowledged that it afforded, when rightly used, a means of making far-reaching human progress of a kind that must complement purely cultural advancement. Beyond that, genetic improvement was even affirmed to be a right which future generations would consider those of the past who were aware of the situation as having been obligated to accord them, just as they in turn would consider themselves as being similarly obligated to their own successors. This obligation would not be regarded as a burden, however, but rather as a high privilege and a challenge to their creativity. At the same time, it was recognized that adequate implementation of eugenic policies also required a clearing away of the ancient heritage of superstition and taboos that hitherto had so obstinately enshackled human usages and preconceptions in matters of sex and reproduction.

It is true that our own world of today is still grievously beset with the old inequalities, prejudices, and mummuries. However, all peoples have by now seen the handwriting on the wall that spells the end of these irrationalities. For modern technologies have, on the one hand, made it too dangerous for the world to remain divided. They have, on the other hand, provided the means for achieving an unparalleled interdiffusion of techniques, ideas, personnel, education, and socioeconomic organization, and for raising standards of living. In the process, provincialisms are at last being ground down, though not without much friction. Opportunities, educational, economic, and social, are being extended ever more effectively to the more depressed social classes and ethnic groups in our own country and elsewhere. A real effort is being made to bring the viewpoint of science home to the general population. The battles that superstition is still winning take place ever closer and closer to its heartland, as was so well depicted in the moving picture on the Scopes trial, *Inherit the Wind*. And it is even becoming permissible to debate seriously matters that in the days before nuclear weapons, space ships, Kinsey, and the Darwin centenary were taboo among all nice people.

Thus the scene has at last been shifted to such an extent as to make it fitting to re-examine even such a scandalous subject as eugenics, with a view to preparing the new forces now arising in the world to deal with it both

realistically and humanistically. For, as we shall see, cultural progress of the kinds mentioned has already proceeded far enough here and there to make the beginnings of a new approach to the subject possible, and the attitudes now formed and the preparations now made may presently lead, when the time is ripe, to more salutary developments in this field than could ever before have occurred.

Contradictions in the Traditional Eugenic Methods

Let us first examine the methods by which it has hitherto been thought that eugenics might operate. These methods have taken their cue from the natural selection of the past. All evolution has had its direction determined in some way by the force of selection. Selection chooses among the materials available to it, namely, diverse mutations, which occur in a manner that is fortuitous so far as their adaptation to the needs of the organism in the given situation is concerned. Selection acts entirely through differential multiplication, but this process can be conceptually divided into two parts, namely, unequal rates of survival (or, conversely stated, of mortality), on the one hand, and unequal rates of reproduction of the survivors, that is, differential fertility (or, conversely stated, differential infertility), on the other hand.

Eugenists have therefore distinguished between two conceivable methods—differential control over mortality, and differential control over reproductive rate. The first method, however, although practiced by the Spartans and by primitive tribes who destroyed infants regarded as undesirable, is universally acknowledged to be inconsistent with the respect for human beings that forms an essential part of civilization. It might be contended that artificial abortion is an intermediate method, but everyone recognizes this also to be an undesirable means where other procedures are available. Essentially, then, this has left for eugenics the second alternative: that of a qualitatively differential control over reproduction prior to or at conception.

In Galton's time, before the advent of modern contraceptive techniques, it was indeed rather visionary to conceive of people's reproduction being governed in the interests of the progeny. For this could be done only

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VOL. 134

by the drastic method of surgical sterilization, of a type that interfered with the sexual life, or by such consummate self-control as voluntary abstention from intercourse, or from its completion.

The development of techniques for cutting or ligating the tubes that conduct the mature reproductive cells afforded less objectionable means of sterilization, but this procedure was still usually regarded by most people—rightly or wrongly—as too irrevocable, except, perhaps, for persons who were mentally or morally hopelessly irresponsible. For them, enforced operations of this type were legalized in some regions, although it was rightly pointed out that there was grave danger of abuse of the practice unless it were confined to the most extreme cases. For attitudes that seem wrong in one place or setting may seem right elsewhere, and nonconformists may at times have moral standards superior, in a longer perspective, to those of the majority who condemn them. Thus, the amount of sterilization resulting from legal applications of an advisable kind would be so minute as to have very little eugenic influence.

However, the invention of fairly practicable artificial means of voluntary contraception opened up much wider possibilities for the control of reproduction in economically developed countries. As we all know, advantage has been taken of these techniques on a large scale, and they have become one of the indispensable procedures whereby the general standard of living has been so greatly raised. Still more practicable means of contraception seem at last to be on the way, thanks to the efforts of a handful of devoted scientists, and they cannot come too soon, for it is imperative to make similar benefits possible in the less developed regions.

But although contraception that is used for the enhancement of cultural benefits through the control of population quantity is at the same time a potential instrument for the improvement of genetic quality, such improvement does not occur unless the contraception is specifically aimed in this direction. To be sure, this purpose might be achieved if the individual couples concerned were to reach their decisions about how many children to have in a highly idealistic spirit, one guided by almost heroic self-criticism and wisdom. We shall presently consider whether or not it is realistic to

expect this. A second proposal has been that of altering the economic and social system in such a way that people of higher gifts and greater natural warmth of fellow feeling—that is, the genetically more highly endowed—would be normally led into occupations and modes of life more conducive to having a large family. Conversely, the organization of society which this view would hold to be ideal would tend to lead persons less well endowed to choose, of their own accord, situations in life that would encourage them to expend their energies in other pursuits than reproduction, and would give them less inducement for raising families.

These two approaches, the individual and the societal, are, of course, not mutually exclusive, and most 20th-century eugenists have advocated a combination of the two. But let us examine each of them more closely. First, as regards the individual approach, which is supposedly to be adopted by people in general once they have been well educated in matters of evolution and genetics, it should be acknowledged that people in general can in fact be taught to take pride in making great sacrifices for what they recognize to be a great cause, especially when they win social approval thereby. This has often happened in times of war as well as after social revolution. However, it seems asking almost too much to expect those individuals who are *really* less well equipped than the average, in mentality or disposition, to acknowledge to themselves that they are genetically inferior to their neighbors in these respects, and then to publicly admit this low appraisal of themselves by raising no family at all or a smaller one than normal, especially since at the same time they would often be thwarting a natural urge to achieve the deep fulfillments, accorded to their neighbors, that go with having little ones to care for and bring up. Moreover, those with physical impairments would likewise tend to rationalize the situation, by thinking that they possessed some superior psychological qualities that more than compensated for their physical defects.

In fact, then, the ones most likely to comply with the idea of restraining their own reproduction would be those who had such strong social feelings, such a sense of duty, so high a standard of what is good, so little egotism, and such an urge for objectionable, as actually to lean over back-

ward and so underrate themselves. Thus we would be likely to lose for the next generation much of what might have been its best material.

On the other hand, for many of the really gifted there are often unusual opportunities for achievement, for rich experiences, and for service along other lines than those of bringing up a large family. Hence it would be only human of them, even though they were in sympathy with eugenic ideals, to expend a larger share of their energies in these other ways than does the average man or woman, for whom the home is often both a refuge and the chief stage on which to express leadership. In view of these considerations, it is not at all surprising that eugenic practices of this intentional, personal type, that require a correlation between the size of one's family and one's realistically made appraisal of one's genetic endowments, have made so little headway, even where they were approved theoretically. Thus, even among eugenists themselves, one seldom finds much evidence that these principles are being acted upon.

What, then, about the proposal that our society should introduce features into its structure whereby the more gifted, the abler, and the more socially minded would find conditions more conducive to their raising a large family, while those less capable or relatively antisocial would tend automatically to be deflected from family life? Surely we would not want a dictatorship to institute such a system, for dictators are oftener wrong than right in their decisions. Moreover, their subjects are not able to become truly men, in the all-round sense, and those who shine under such circumstances are not likely to be the wise and the responsible.

Under a democracy, on the other hand, is it not likely that "the common man" will refuse to subject himself to such manipulations? Certainly if the proposal took some such crude form as a subsidy for the raising of children, allotted to those who already occupied better positions or who had scored higher on certain tests, it would rightly be resented and defeated as discriminatory by the great majority. And even if subtler forms of influence were used, such as special aids to family life for those in occupations requiring greater skills, responsibility, or sacrifice, there would soon be a clamor on all sides to have these advantages extended to

every responsible citizen. No, we can hardly use democracy to support any kind of aristocracy. To be sure, ways might eventually be found to reduce the present strong negative correlation between educational or social achievement and size of family, and these would be all to the good. But no major formula is in sight for restoring the greater family size of the fitter, while retaining that most essential feature of our culture—the extension to all of mutual aid based on the most advanced technologies available.

It might seem to follow that we have now, as a result of our improved techniques for living, reached an inescapable genetic cul-de-sac. It might be concluded that we should therefore confine ourselves entirely to the immediate job on hand—the pressing and rewarding one of all social reformers and educators—that of making the best of human nature as it is, the while allowing it to slide genetically downhill, at an almost imperceptible pace in terms of our mortal time scale, hoping trustfully for some miracle in the future.

The New Approach: Germ-Cell Choice

However, it is man who has made the greatest miracles of any species, and he has overcome difficulties arising from his technologies by means of still better basic science, issuing in still better technologies. And so in the case of the genetic cul-de-sac of the present day, he has even now possessed himself of the means of breaking through it. For he is no longer limited, like species of the past which had the family system, to the two original methods of genetic selection applying to them: that of differential death rate on the one hand, and differential birth rate or family size, on the other hand. He has now given himself, in addition, the possibility of exerting conscious selection by making his own choice of the source of the germ cells from which the children of his family are to be derived (3-5). At present, this choice is confined to the male germ cells, but there are indications that with a comparatively small amount of research it might in some degree be extended to those of the female as well.

It is pretty common knowledge nowadays that some tens of thousands of babies have already been born, in the United States alone, that were derived

by "AID," that is, by artificial insemination with sperm obtained by the physician from a donor chosen by him, but whose identity was kept unknown to all others, including even the parents. In the great majority of these cases the husband had been found to be irredeemably sterile. And although, in view of the prevalence of the traditional mores, the whole matter was kept secret from the children themselves, both members of the couple had in these cases been eager to avail themselves of this opportunity to have one or more children. This method has, of course, been allowed only to those likely to make good parents—or, shall we say, good "love parents," as distinguished from "gene parents"? Moreover, follow-up studies have shown that these parents did truly love their children, as the children did their parents. It is noteworthy that this proved to be as much the case for the father as for the mother, and that the marriage was strengthened thereby.

Here, then, we see repeated what is typically found in those cases of early adoption in which the children have been genuinely desired. However, this "pre-adoption" (as Julian Huxley has termed it) is likely to prove even more binding and satisfying than "post-adoption." And the method of pre-adoptional choice, despite its relative crudity at the present time, has demonstrated its capability of producing a superior lot of children. Thus, the couples that practice it have made a virtue of necessity by inducing the genesis of children of whom they can usually be even prouder than of the children they would have had if they had been free from reproductive infirmity.

Recently, as more people have become alive to matters of genetics, an increasing number of couples have resorted to AID when the husband, although not sterile, was afflicted with some probably genetic impairment, or likely to carry such an impairment that had been found in his immediate family. Similarly, those with incompatibilities in blood antigens have also made use of the method with good results. In these ways a beginning has already been made in the conscious selection of germinal material for the benefit of the progeny. It is to be expected that many more people will seek such advantages for their prospective children when there are available for creating them the germ cells of persons who are decidedly superior

in endowment to the normally healthy and capable persons that are commonly sought as donors by the physicians of today. For there is no physical, legal, or moral reason why the sources of the germ cells used should not represent the germinal capital of the most truly outstanding and eminently worthy personalities known, those who have demonstrated exceptional endowments of the very types most highly regarded by the couple concerned, and whose relatives also have tended to show these traits to a higher-than-average degree. How happy and proud many couples would be to have in their own family, to love and bring up as their own, children with such built-in promise.

Today, of course, most people have such an egotistical, individualistic feeling of special proprietorship and prerogative attaching to the thought of their own genetic material as to be offended at the suggestion that they might engage in such a procedure. No one proposes that they do so as long as they feel this way about the matter. However, they should not try to prevent others who would welcome such an opportunity from ordering their lives in accord with their own ideals. And as the prejudice against the practice gradually dwindles, the manifest value of the results for those who had participated in it would appeal to an increasingly large portion of the population.

In this connection it is important to bear in mind that there is no such thing as a paternal instinct in the sense of an inherent pride in one's own genetic material or stirps. Some primitive peoples, even including a few still in existence in widely separate regions, have had, strange as it may seem to us, no knowledge that the male plays a role in the production of the child, much less have they had any conception of genetic material or genes. Thus, among some of them the mother's brother has effectively filled the role of father in regularly caring for mother and children. Moreover, among some peoples, such as the Hawaiians, children are rather freely adopted at an early age into other families, into whose bosom they are warmly, fully, and unambiguously accepted as equals in every way to the natural children. It is "second nature," but not "first nature," for us in our society to exalt our own stirps.

It is, however, "first nature" for

men and women to be fond of children and to want to care for them, and more especially, those children with whom they have become closely associated and who are dependent on them. If the love of a man for his dog, and vice versa, can go to such happy lengths as it often does, how much stronger does the bond normally become between the older and younger generations of human beings who live together. And since, in the past, the children have usually been those of the parents' own stirps, it has been a natural mistake to suppose that these stirps, rather than the human associations of daily life, formed the chief basis of the psychological bonds that existed between parent and child. Yet, as our illustrations have shown, this view is incorrect, and a family life of deep fulfillment can just as well develop where it is realized that the genetic connection lies only in our common humanity.

The wider adoption of the method of having children of chosen genetic material rather than of the genetic material fate has chanced to confer on the parents themselves implies, of course, that material from outstanding sources become available by having it stored in suitable banks (3, 6, 7). It would be preferable to have it in that glycerinized, deep-frozen condition developed by modern technology, in which it remains unchanged for an unlimited period without deterioration. It is true that research is badly needed for finding methods by which immature germinal tissue can, after the deep-freezing which it is known to survive, be restored to a state where it will multiply *in vitro* and subsequently produce an unlimited number of mature reproductive cells. Even without this further development, however, the way is already open, so far as purely biological considerations are concerned, for gathering and inexpensively storing copious reserves of that most precious of all treasures: the germinal material that has formed the biological basis of those human values that we hold in highest regard.

The high potential service to humanity represented by the pre-adoption of children should carry with it the privilege, for the parents, of having a major voice in choosing from what source their adopted material is to be derived. Surely, if they have ever had the right to produce, willy-nilly, the children that would fall to their

lot as a result of natural circumstances, they should have the right of choice where they elect to depart from that haphazard method. They certainly would not wish knowingly to propagate manifest defectives, and, being idealistic enough to undertake this service at all, they would in most cases be glad to give serious consideration to the best available assessments of the genetic probabilities involved, as well as be open to advice regarding relative values and needs.

It would be made clear to parents that there is always an enormous amount of uncertainty concerning the outcome in the genetics of an organism so crossbreeding as man, especially since the most important traits of man are so greatly influenced by his cultural environment. Nevertheless, facing this, they would realize that the degree of promise was in any such case far greater than for those who followed the traditional course. It would be in full awareness of this situation that they would exercise their privilege of casting the *loaded* dice of their own choosing.

This kind of choice means that the physician can no longer be the sole arbiter of destiny in this matter. Clearly, if the couple are to accept their share of the responsibility and privilege here involved, the practice of keeping the donor unknown to them must be relinquished in these cases. Knowledge of the child's genetic lineage will also be needed later, so that sounder judgments may be reached concerning his genetic potentialities in the production of the generation to follow his own. This lifting of the veil of secrecy will become ever more practicable, and in fact even necessary, as the having of children by chosen genetic material becomes more widely accepted and therefore more frequent. Moreover, the attitude of others toward the couples who have employed this means of having their children will gradually become one of increasing acceptance and then of approbation and even honor.

Today's fear that knowledge by the mother of the identity of the gene father may lead to personal involvement between the two, to the detriment of normal family life, will recede when the gene source is remote in space or time, as when the germinal material has been kept in the deep-frozen state for decades. This procedure will also allow both the individual

worth of those being considered as donors, and their latent genetic potentials, to be viewed in better perspective, and will reduce the danger that choices will be based on hasty judgments, swayed by the fads and fashions of the moment.

Let us see in what ways this method of reproduction from chosen material tends to avoid the difficulties that are encountered in attempting to reconcile traditional reproduction with the interests of genetic quality by somehow controlling the size of families. For one thing, as previously pointed out, most men would resist accepting and acting on the conclusion that they are below their next-door neighbor, or below average, in genetic quality. They would particularly resist the idea that they themselves are in that lowest fifth which would be required to refrain from having children if an equilibrium of genetic quality were to be maintained in the face of a 20-percent mutation rate. Yet most of these same people would willingly accept without resentment the idea that they are not among the truly exceptional who conform most closely to their own ideals. And so, when encouraged by the community mores, they would be glad and proud to have at least one of their children derived, by choices of their own, from among such sources. Thus they would continue to have families of a size more nearly conforming with their inclinations.

On the other hand, the worthy but humble, those who might otherwise, from overconscientiousness, limit their families unduly, would often be eager to serve as love parents. And although in that capacity they would tend to derive the germ cells from outside sources, they would be especially likely to have a well developed sense of values and so to choose sources even worthier than themselves. Finally, the really highly endowed but realistic would not be confronted with the sore dilemma of choosing between exercising their special gifts, on the one hand, or having the large family to which genetic duty seemed to obligate them, on the other hand. For their germinal material would tend to be sought by others, if not in their own generation, then later, and to a degree more or less in proportion to their achievements. Thus they would be freed to give their best services in whatever directions they elected.

In all these ways, the diverse ob-

OL. 134

stacles encountered by a eugenics that tried to function by means of a consciously differential birth rate—that is, by adjustment of family size—would be avoided. Thereby, a salutary separation would be effected between three functions that often have conflicting needs today. These are, first, the choice of a conjugal partner; this should be determined primarily by sexual love, companionability, and compatible mentality and interests. Second, there is the determination of the size of the family; this should depend largely on the degree of parental love that the partners have, and on how successfully they can express it. Third, there is the promotion of genetic quality, both in general and in given particulars; these qualities are often very little connected with the first two kinds of specifications. By thus freeing these three major functions from each other, all of them can be far better fulfilled. Under these circumstances the conjugal partners need not be chosen by criteria in which a compromise is sought between the natural feelings and considerations of eugenics. Neither need the family size be restricted or expanded according to eugenic forebodings or feelings of duty. Yet at the same time there can be a far more effective differential multiplication of worthy genetic material than in any other humanly feasible way.

Further Prospects

It is likely that the avoidance of the effects of sterility will not be the only door through which such a change of mores will be approached. Facilities for keeping germ cells, suitably stored below ground in a deep-frozen condition, in areas relatively free from radiation and chemical mutagens, may well be provided in our generation for an increasing number of people (8). Among these would be persons subject to the growing radiation hazards of industry, commerce, war, and space flight, and those exposed to the as yet unassessed hazards of the chemical mutagens of modern life. The same means would greatly retard the accumulation of spontaneous mutations which probably occurs during ordinary aging. Thus, wives may in time demand such facilities for the storage of their husbands' sperm. These facilities would be provided not only for the sake of reducing mutational damage but also as a kind of insurance in the event of the

husband's death or sterility. In these ways great banks of germinal material would eventually become available. They would be increasingly used not only as originally intended but also for purposes of conscious choice. Moreover, some of these stocks might become recognized as especially worthy only after those who had supplied them had passed away.

The cost of storage is, relatively, so small that failure to make such a provision will eventually be considered gross negligence. As Calvin Kline (7) points out, this will be especially the case where (as in India today) voluntary vasectomy becomes more prevalent as the surest and, in the end, the cheapest means of birth control. For when vasectomy is complemented by stores of sperm kept *in vitro*, the process of procreation thereby achieves its highest degree of control—control not subject to the impulses of the moment but only to more considered decisions.

It is true that most people's values, in any existing society, are not yet well enough developed for them to be trusted to make wise decisions of the kind needed for raising themselves by their bootstraps, as it were (9). But this type of genetic therapy, of "eutelegensis," as Brewer termed it when he advocated it in 1935 (4), is certainly not going to spring into existence full fledged overnight. It will first be taken up by tiny groups of the most idealistic, humanistic, and at the same time realistic persons, who will tend to have especially well developed values. This mode of origination of such practices was pointed out by Weinstein in "Palamedes" in 1932 (10). These groups will tend to emphasize the most basic values that are distinctive of man, those that have raised him so far already, but which still may be enormously enhanced. Foremost among these are depth and scope of intelligence, curiosity, genuineness and warmth of fellow feeling, the feeling of oneness with others, joy in life and in achievement, keenness of appreciation, facility in expression, and creativity.

Those who follow these lodestars will blaze the trail, and others will follow and widen this trail as the results achieved provide the test of the correctness with which its direction was chosen. Meanwhile, the world in general, through its reorganizations of society and education, is moving in the same direction, by a de-emphasis of its provincialisms and a consequent recog-

nition of the supreme worth of these basic human values. For after all, these values have been prominent in the major ethical systems of the whole world.

At the same time, plenty of diversity will inevitably be developed. For each especially interested group will naturally seek to enhance its particular proclivities, and this is all to the good. But on the whole, the major gifts of man have been found to be not antagonistic but correlated. Thus we may look forward to their eventual union with one another in a higher synthesis. And from each such synthesis in turn, divergent branches will always be budding out, to merge once more on ever higher levels.

It may be objected that we have next to no knowledge of the genes for those traits we value most, and that their effects are inextricably interwoven with those of environment. As was acknowledged earlier, this is quite true. However, it has also been true in all the natural selection of the past and in the great bulk of artificial selection. Yet these empirical procedures, based entirely on the accomplishment of the individuals concerned, did work amazingly well. We can do a good deal better by also taking advantage of the evidence from relatives and progeny. Yet that evidence also is furnished mainly by accomplishments or output. Where those were high, the environment was, to be sure, usually favorable, but so was the heredity. And as, in our human culture, social reform proceeds and opportunities become better distributed, our genetic judgments will become ever less obscured by environmental biases, while at the same time our knowledge of genes will improve.

Meanwhile, the efforts of educators and the lessons of world affairs will serve to emphasize the same values for us. And these attitudes we will take over for our genetic judgments also. In this connection, another consideration deserves mention here. The preference which most parents will inevitably have for the genes of persons of truly remarkable achievement and character, rather than for those of the merely eminent or powerful, will at the same time serve to direct the stream of genetic progress toward the factors underlying creativity, initiative, originality, and independence of thought, on the one hand, and toward genuineness of human relations and affections, on the other hand. Otherwise the genetic movement might, as so often happens in other affairs of men, become directed

toward skill at conformity, showmanship, and the dignified hypocrisy that often brings mundane success and high position (9, 10). This would have been a far greater danger in the case of the old-style eugenics.

But, it may be objected, does all this really represent conscious control in an over-all sense? Is it not merely a type of floating along in a chaotic manner, each straw making its own little movement independently of the rest, without a general plan or goal or stream? The answer is that humanity is as yet too limited in knowledge and imagination, too undeveloped in values, to see more than about one step ahead at a time. That step, however, can be discerned clearly enough, and by enough people, to give rise to a general trend in

a salutary direction. And at the higher level to which each step taken will bring us we will be able to see an increasing measure of advance ahead. So we humans will achieve, not through dictation but through better general understanding and ever more clearly seen values, increasing mutual consent both concerning the means to be used and the aims toward which to orient. Thus an ever wider over-all view will emerge, and a surer, greater over-all plan, or rather, series of plans. To create them and to put them into effect will then enlist our willing efforts. And the very enjoyment of their fruits will bring us further forward in our great common endeavor: that of consciously controlling human evolution in the deeper interests of man himself (11).

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Optical Rotatory Dispersion

Investigation of the phenomenon 140 years after its discovery sheds light on problems of organic chemistry.

Carl Djerassi

For instance, in the field of infrared spectroscopy, this interval amounted to approximately 40 years, while less than a dozen years intervened between the discovery of the nuclear magnetic resonance phenomenon and its extremely wide use in organic chemistry.

Early Studies

The development of optical rotatory dispersion followed precisely the same path, except that nearly 140 years elapsed between the original discovery by Biot (1) of the change of optical rotation of quartz with wavelength and the actual application of this general phenomenon to organic chemical problems (2). In the interval, a substantial amount of research in this area was conducted largely by physical chemists, and reviews (3) by three of the pioneers—Lowry, Levene, and Kuhn—show on the whole a rather understandable preoccupation with theoretical aspects of optical rotation and rotatory dispersion. During this entire period, up to around 1954, considerably less than 100 optical rotatory-dispersion curves had been recorded in the ultraviolet region of the spectrum, principally because of the instrumental difficulties involved in such studies. Indeed, between the 1930's—when experimental work by these three

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The organic chemist, notably the investigator in the natural products field, has always been very ready to use new physical tools for the solution of his problems. Nowadays, it is difficult to conceive of modern organic chemical research being conducted without the use of ultraviolet, infrared, or nuclear magnetic resonance spectroscopy, and it is profitable to consider for a moment how these tools became acceptable to the organic chemist. Invariably, the initial discovery was made by the physicist or physical chemist, who was usually concerned only with the phenomenon itself, rather than with its application to the more mundane everyday problems of the organic chemical research laboratory. Application in the laboratory of the organic chemist occurred only when instrumentation became sufficiently advanced or simplified so that a relatively large number of measurements could be conducted on organic compounds. These measurements then lent themselves to empirical correlation with some structural feature of the organic molecule, and this invariably led to two developments—one, a rapid acceptance by other organic chemists, with a consequent enormous increase in the number of measurements and hence refinements in empirical correlation, and the other, a revival (or continuation) of interest in the theoretical aspects of this particular tool and a more fundamental explanation of the generalities uncovered by the semiempirical studies of the laboratory chemist.

This has been the history of virtually all physical methods which have found a temporary or a permanent place in organic and biochemical research; only the time lag between the initial physical discovery and the first widespread use by the organic chemist has differed.

research groups (3) had terminated—and 1955, only a handful of publications appeared on ultraviolet optical rotatory-dispersion measurements, and there are practically no instances on record where this method was used for the solution of an organic or biochemical problem. This state of affairs should be contrasted with two striking indications of revived activity and interest in optical rotatory dispersion, especially as it applies to organic chemistry and biochemistry. The advances in instrumentation are illustrated by the observation that in our laboratory at Wayne University and later at Stanford University, approximately 3000 ultraviolet rotatory-dispersion curves were measured in less than 8 years, while a quantitative index of general acceptance of optical rotatory dispersion as a physical tool is afforded by the fact that during the past 18 months approximately 200 publications have appeared in which ultraviolet optical rotatory-dispersion measurements have been employed for the solution of some organic chemical or biochemical problem.

General Principles

To appreciate the reasons for this renaissance, a brief discussion of optical rotatory dispersion, in terms of its dispersive and absorptive character, is required. We may consider a beam of plane-polarized light to be made up of a right and a left circularly polarized component. Although no rotation will be observed if these two components pass through a medium with equal velocity, the plane of polarization of the emergent light will be rotated if the refractive indices of the medium for right and left polarized light are unequal. This dispersive feature is the principal factor in monochromatic polarimetry, as, for instance, in the conventional measurement of optical rotation of an optically active substance at one wavelength, such as the sodium D line (5890 Å). In addition to unequal velocity of transmission, there may also be unequal absorption of left and right circularly polarized light. The emergent light will then be elliptically polarized, and this absorptive aspect is called circular dichroism. The combined phenomena of circular dichroism (unequal absorption) and optical rotation (unequal velocity of transmission) are named the "Cotton effect," after its

discoverer. As will be shown, it is the experimental measurement of this Cotton effect and its correlation with certain structural and stereochemical features that is chiefly responsible for the application of optical rotatory dispersion in organic chemistry.

In principle, the experimental determination of an optical rotatory-dispersion curve is a simple matter that involves the measurement of optical rotation of a given substance at different wavelengths, rather than just at the conventional sodium D line. All that is required is a light source, a monochromator for yielding light of a given wavelength, a polarizer, a cell containing the optically active material in solution, an analyzer, and a device for measuring the angle of rotation. The polarizer and analyzer are usually quartz Rochon prisms, and the angle of rotation is measured visually when one is working with visible light (such as the sodium D line). However, most organic compounds are colorless and absorb only in the ultraviolet region of the spectrum. Consequently, if one wishes to measure experimentally the Cotton effect of colorless substances, then one must carry out optical rotation determinations in the ultraviolet spectral range. This cannot be done visually, and it is here that experimental difficulties arise. In the earlier work (3), determinations in the ultraviolet region were made with laborious photographic procedures, and it was only the development of a commercially available spectropolarimeter (see 2, chap. 3) that made possible the relatively rapid and simple measurement of a rotatory-dispersion curve in the ultraviolet. With the spectropolarimeter the measurement could be made in one or two hours, whereas the photographic method required several days.

Plain and Anomalous Curves

There are two types of dispersion curves (2, chap. 2; 4). Figure 1 illustrates some plain curves—that is, dispersion curves that exhibit no "maxima" or "minima" but a simple increase in rotation as one proceeds into the ultraviolet. Curves A and B of Fig. 1 are called positive plain curves even though curve B starts out on the negative rotation side in the visible and crosses the zero rotation axis in the ultraviolet; curve C is called a negative plain curve.

An anomalous optical rotatory-dis-

persions curve is one that exhibits rotational maxima and minima; an idealized example is reproduced in Fig. 2. Anomalous curve A represents a single positive Cotton effect, while curve B is typical of a negative Cotton effect, since the first extremum in going from the visible to the ultraviolet is a rotational minimum. The rotational maxima and minima are called peaks and troughs (2, chap. 2; 4), so as not to confuse them with ultraviolet absorption maxima and minima. Thus, the positive Cotton effect of curve A in Fig. 2 would be exhibited by an optically active substance with a chromophore absorbing at wavelength λ_0 , which represents approximately the mid-point in terms of wavelength units between the positions of the peak and the trough of the rotatory-dispersion curve. It is this intimate relationship between the absorption spectrum and the optical rotatory dispersion of a given substance that usually indicates whether the substance will exhibit an anomalous rotatory-dispersion curve in a given wavelength region.

Thus, plain curves of the type shown in Fig. 1 will be exhibited generally by optically active substances (for example, hydrocarbons, alcohols, and carboxylic acids) that do not absorb in an accessible region of the spectrum, while an anomalous curve (for example, A and B in Fig. 2) will be shown by a substance containing a chromophore (such as a carbonyl or nitro group) that does absorb in such a region. Actually, every plain curve will eventually become anomalous as it approaches its optically active absorption band, but in many compounds these bands may occur so far down in the ultraviolet, or may absorb with such intensity, that rotation measurements are not feasible in that spectral region. To illustrate this point, the anomalous rotatory-dispersion curves shown in Fig. 2 are actually plain curves over the range 7000 to 4500 Å and become "anomalous" only as they approach the region of the absorption maximum ($\lambda_0 = 3000$ Å).

An obvious conclusion from either the plain or the anomalous dispersion curves shown in Figs. 1 and 2 is that optical rotations will invariably be much greater in the ultraviolet than in the visible, and that the historical choice of the sodium D line at 5890 Å for monochromatic rotation measurements is a rather poor one. Much smaller amounts of optically active substrate

are required for rotation measurements in the ultraviolet than in the visible, and most of the ultraviolet optical rotatory-dispersion measurements in our laboratory have been conducted with 0.1 to 1.0 milligram of material, a quantity that is usually totally inadequate for rotation measurements at the sodium D line.

Between 1812 and 1954, only approximately 50 Cotton-effect curves had been measured experimentally (3). Among the various chromophores that had been investigated there were a few ketones, such as camphor, whose rotatory-dispersion curve was similar to curve A in Fig. 2. While this experimental material had not led to any widespread applications, it nevertheless demonstrated the feasibility of measuring Cotton effects, provided substances with suitable chromophores are selected. Therefore, when the first commercially available model of the Rudolph spectropolarimeter was delivered to our laboratory, we set out to test certain assumptions.

Applications to Stereochemistry

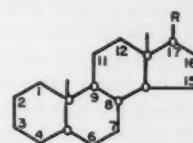
While plain rotatory-dispersion curves (Fig. 1) are preferable for stereochemical correlation (2, chap. 16; 5) over single rotation measurements at the sodium D line, especially when the curves cross the zero line (curves A and B in Fig. 1), they are obviously

limited in scope. On the other hand, if variations in the asymmetric environment of a given spectroscopically accessible chromophore should result in Cotton-effect curves of different shape or sign, and if these differences could be related to certain structural or stereochemical features of the molecule, then a valuable physical tool would be available to the organic chemist.

For initial experimental examination we selected the carbonyl chromophore, since it absorbs in an accessible spectral region (2800 to 3300 Å), with a rather low extinction that would permit rotation measurements through this region of maximal absorption, an indispensable feature if Cotton effects are to be determined experimentally. Furthermore, earlier work (3) on camphor and a few related terpene ketones had shown that such ketones exhibit Cotton-effect curves—in other words, that the intrinsically symmetrical carbonyl chromophore $\text{C}=\text{O}$ became "optically active" when placed in an asymmetric environment—although it could not be determined from these few studies whether the Cotton effect would reflect structural changes in its asymmetric surroundings. A further reason for selecting the carbonyl chromophore was the ready availability of many optically active ketones and aldehydes, or at least of alcohols from which they could be obtained by oxidation.

With the choice of the chromophore—that is, the "absorptive" component

of the rotatory dispersion—settled, it was then necessary to decide upon a reasonable asymmetric framework that would offer sufficient scope for selective alteration to find whether such changes would result in variations in shape or sign of the Cotton effect. For this purpose we chose a group of compounds that was uniquely suitable, the steroids, which are characterized by the following skeleton:



The advantages of this group of compounds are manifold. All of them are optically active, of known absolute configuration, and usually of fixed and known conformation. Furthermore, probably several thousand representatives of this class have been prepared during the past 30 years, largely because of the great biological importance of the steroids. Finally, and perhaps most importantly for our purposes, most of the steroids possess a chain of seven contiguous asymmetric carbon atoms (circled in the structure shown above), so that irrespective of where a carbonyl group is placed in this molecule, it can, at best be only one carbon atom away from this asymmetric chain. When a carbonyl group is placed in the

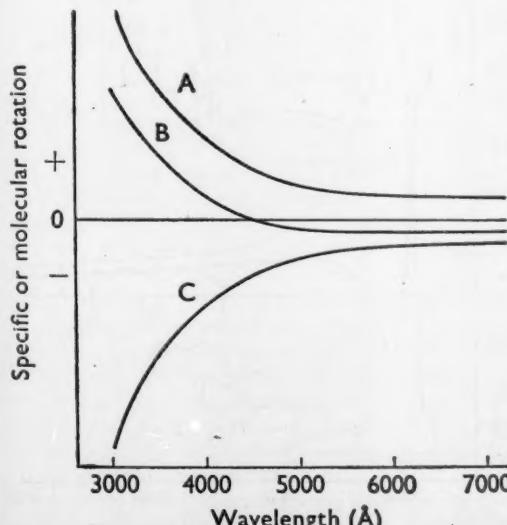
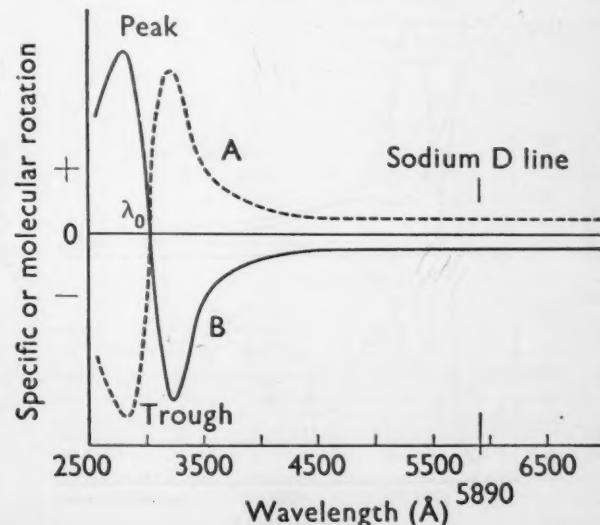


Fig. 1 (left). Typical plain rotatory-dispersion curves: A and B, positive; C, negative. Fig. 2 (right). Typical anomalous rotatory-dispersion curves: A, positive Cotton effect; B, negative Cotton effect.



eleven possible locations of the four-ring skeleton, not only will the chromophore find itself in different asymmetric situations but the incorporation of a trigonal carbon atom into any one of the rings will cause a displacement of the valency angles with a concomitant ring-geometry distortion that may make itself felt through varying ranges of this asymmetric backbone.

Since many such steroids and a modern spectropolarimeter were available in our laboratory, we were able to measure, in a rather short time, many hundreds of ultraviolet rotatory-dispersion curves of closely related ketones. This initial work (6) demonstrated that variations in the shape and sign of such Cotton effects can indeed be related to many important molecular parameters. This prompted us to carry out a much more extensive investigation of the application of optical rotatory-dispersion measurements to organic chemistry than had been contemplated initially. Much of this work appeared between 1955 and 1960, in some 40 articles from our laboratory, and I have recently summarized it in book form (2). Therefore, I will give only a few highlights below, to indicate the general scope of the method and the directions of future research.

Structural Applications

Figure 3 may be considered a typical example of structural information that can be derived from a rotatory-dispersion curve. It contains the dispersion curves of three isomeric cholestanones, distinguished only by the location of the carbonyl group; it may be noted that these curves differ significantly in either shape or sign. Most importantly, it was observed that these characteristic curves are retained in the presence of many other substituents (esters, acids, alcohols, lactones, and so on) that do not absorb in the same spectral range as the carbonyl group. Consequently, we have now available a tool which can frequently tell us, with less than 1 milligram of recoverable substance, where a keto group (and hence the corresponding hydroxyl function) is located in an unknown steroid. At present, no other physical method affords this information.

The differences in the rotatory-dispersion curves of Fig. 3 were brought about by moving the carbonyl group around the asymmetric framework and thus placing it each time in a distinct asymmetric environment; the change is thus reflected in the dispersion curve. Alternatively, such a change in environment may be produced by keeping the

chromophore in the same location but altering the immediate stereochemistry. Thus, reduction of the double bond of the male sex hormone testosterone affords two isomers, 5α - and 5β -androstan-17 β -ol-3-one, differing only in the nature of the A/B ring juncture. As shown in Fig. 4, the 5α -isomer exhibits a positive (A), and the 5β -isomer, a negative (B), Cotton effect. These characteristic rotatory-dispersion features remain unchanged, even if the substituents at C-5 or C-10, or at both positions, in these molecules are replaced by other "nonchromophoric" moieties (CH_2OH , CN , $\text{CH}_2\text{CO}_2\text{CH}_3$, and so on), and this has proved to be very useful in the steroid field for determining the nature of the A/B ring juncture in an unknown 3-keto steroid.

When the carbonyl group is placed next to the ring juncture, then equilibration between the two isomers is possible in the presence of acid or base. This is illustrated in Fig. 5 with the two 15-ketones of the bile acid series, which differ only in the configuration at C-14. The C/D *cis* isomer (B), with a negative Cotton effect, is the more stable isomer, and the kinetics of the isomerization can be determined very conveniently on a micro scale by taking advantage (7) of the information con-

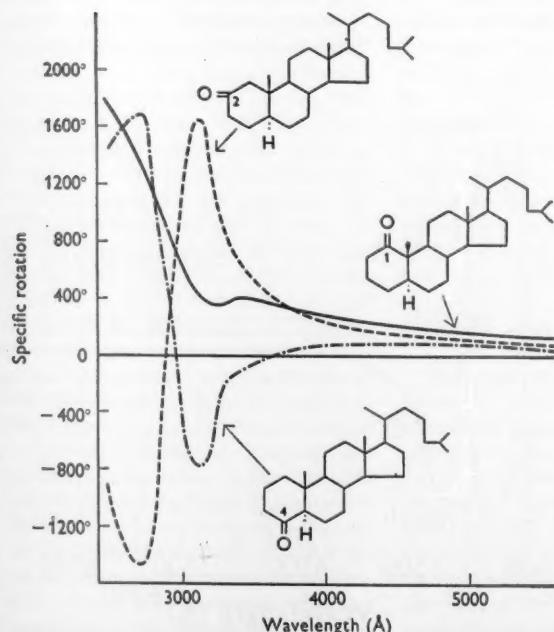
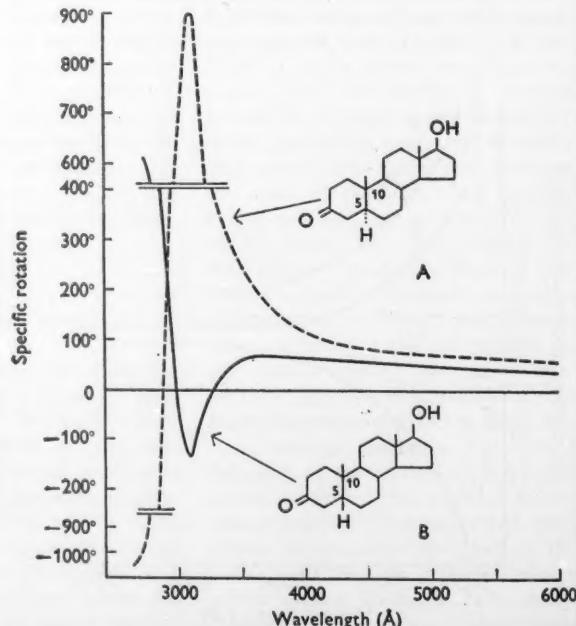


Fig. 3 (left). Optical rotatory-dispersion curves of some isomeric cholestanones. Fig. 4 (right). Optical rotatory-dispersion curves of (A) 5α -androstan-17 β -ol-3-one (dihydrotestosterone) and (B) 5β -androstan-17 β -ol-3-one.



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tained in Fig. 5. It may be noted that the rotations of the two isomers are rather similar at the conventional sodium D line in the visible (5890 Å), while in the ultraviolet near 3200 Å this difference in specific rotation amounts to over 3000 degrees. All that is necessary, therefore, is to measure the rate of change in rotation at some convenient wavelength in the ultraviolet and calculate from these data the kinetics and position of equilibrium.

The rotatory-dispersion curves in Figs. 3, 4, and 5 represent typical examples from the steroid series of structural, stereochemical, and analytical-kinetic applications of dispersion measurements. That these generalizations from the steroid group are, in fact, of much wider scope was then demonstrated (8) by a process of dissection, through which it was found that only

the immediate asymmetric bicyclic environment around the carbonyl group is responsible for the characteristic rotatory-dispersion features. This conclusion led immediately to the two most important applications of rotatory dispersion—the assignment of absolute configurations and the detection of conformational alterations.

Conformational Analysis

The use to which this tool can be put in conformational analysis is illustrated in Fig. 6. *Trans*-10-methyl-2-decalone (I) represents the bicyclic analog of a 5α -3-keto steroid (for example, A in Fig. 4), and its conformation can be defined unambiguously as the all-chair form IA (Fig. 6). The rotatory-dispersion curve of this ketone I proved to be

practically identical with that of steroid A in Fig. 4.

A considerably more difficult situation prevails in the isomeric *cis*-10-methyl-2-decalone (II in Fig. 6), where two all-chair conformations (IIA and IIB) have to be considered. Of these, IIA is called the steroid conformation, since steroids are frozen in this conformation by virtue of the additional ring fusions. Since we had already established (8) that the more distant rings in the steroid series do not play an important part insofar as the typical shape of the rotatory-dispersion curve of a given ketone is concerned, the negative Cotton effect of steroid B in Fig. 4 will thus serve as a suitable reference curve for the steroid conformation of the bicyclic ketone II in conformation IIA (Fig. 6). It may be seen that in the nonsteroid conformation IIB, the

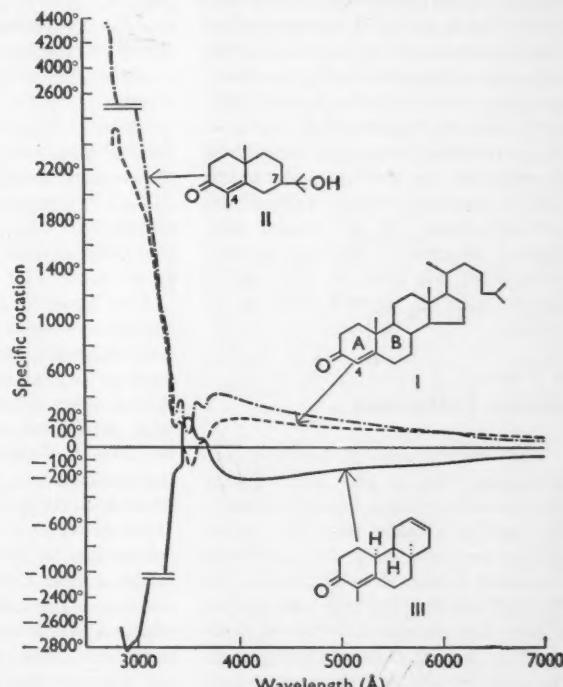
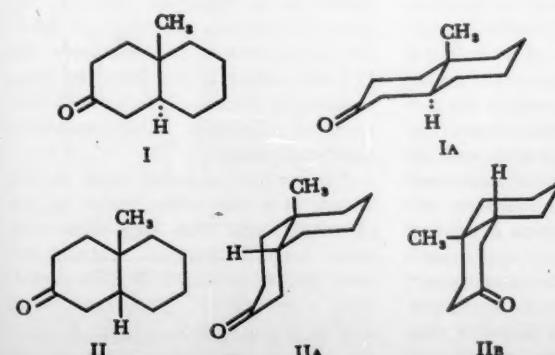
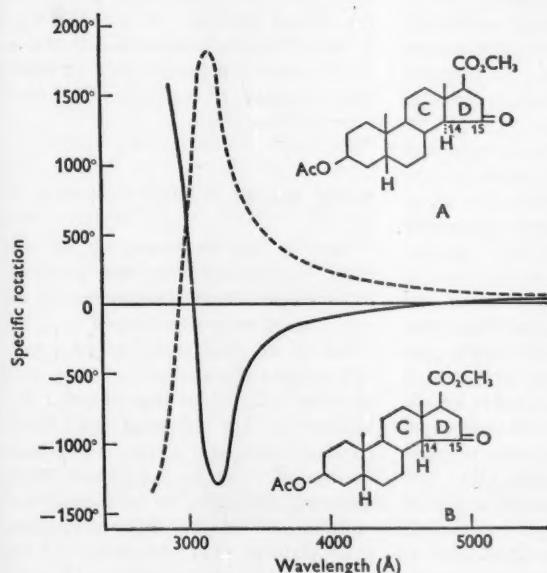


Fig. 5 (top left). Optical rotatory-dispersion curves of (A) methyl 3 β -acetoxy-15-oxoetianate and (B) methyl 3 β -acetoxy-15-oxo-14 β -ethanate. Fig. 6 (bottom left). Conformational representations of (I) *trans*-10-methyl-2-decalone and (II) *cis*-10-methyl-2-decalone. Fig. 7 (right). Optical rotatory-dispersion curves of (I) Δ^4 -cholest-3-one; (II) carissoine; and (III) (—)-1,14-dimethyl-2-oxo- $\Delta^1,6$ -decahydrophenanthrene.

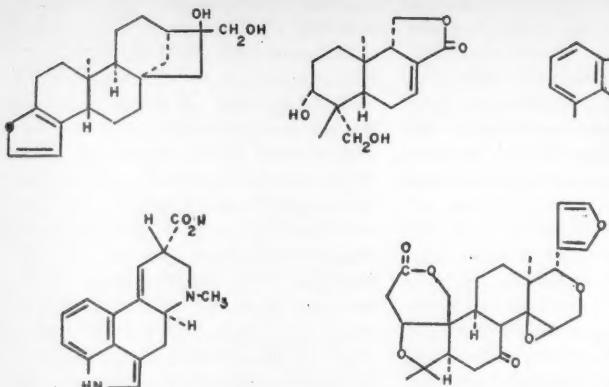


Fig. 8. Configurations of various compounds, established through optical rotatory-dispersion analysis: (top left) cafestol; (top middle) iresin; (top right) allogibberic acid; (bottom left) lysergic acid; (bottom right) limonin.

carbonyl group is in a different asymmetric environment, and its rotatory-dispersion curve would be expected to be quite distinct from that of the steroid model. When ketone II was synthesized (8) in optically active form and when its rotatory-dispersion curve was measured, the curve proved to be very similar to that of the steroid B in Fig. 4. One can deduce, therefore, that ketone II exists in the steroid conformation IIA—a conclusion which, *a priori*, was not self-evident. At the present time, rotatory dispersion is the only physical method that has shed any light on the conformation of such a decalone in solution.

Absolute Configuration

The determination of absolute configuration—that is, the differentiation between mirror-image representations—has been of concern since the earliest days of stereochemistry. The only direct procedure is that of x-ray analysis (9), but until now this has been used in only a very few instances. What is really needed is a quick method which permits the establishment of the absolute configuration of a given molecule by relating it to another reference compound whose absolute configuration has been settled directly or indirectly (through various chemical transformations) by the x-ray method. It is in this area of stereochemistry that optical rotatory-dispersion measurements have filled a very important need.

In principle, the approach (8) is quite simple and is based on the premise employed in Fig. 6 for examining the

conformation of *cis*-10-methyl-2-decane. Since we have shown that the characteristic shape of a rotatory-dispersion curve is governed by the bicyclic stereochemical (and conformational) environment of the chromophore—in the present instance, the carbonyl group—and since additional nonchromophoric substituents do not interfere, then all that is necessary is to compare the rotatory-dispersion curve of the unknown compound with that of a known reference substance, possessing the same chromophore, in a similar environment.

The absolute configuration of the steroids is known (10), and their rotatory-dispersion curves, therefore, can be used for such reference purposes. A typical example is reproduced in Fig. 7, with the rotatory-dispersion curves of the steroid Δ^4 -cholest-3-one (I) and the sesquiterpene carissoone (II). The hydroxy-isopropyl substituent at C-7 of carissoone represents a nonchromophoric substituent, as does the extra methyl group at C-4. This latter point was established (8) in a separate experiment in which it was noted that the rotatory-dispersion curve of Δ^4 -cholest-3-one (I) was not altered in any of its essential details by the addition of a methyl function at C-4. Consequently, the carbonyl chromophore in the steroid I of known absolute configuration and the sesquiterpene carissoone (II) are found in identical bicyclic environments, and as their rotatory-dispersion curves are very similar in shape and sign, there is no question but that the absolute configuration of carissoone is that represented in stereoformula II, rather than its mirror image. The third substance in

Fig. 7, the tricyclic ketone III, also has the same relative stereochemical environment in rings A and B as the steroid I. Its rotatory-dispersion curve, however, is essentially of mirror-image type, and from this it follows that, in terms of its absolute configuration, ketone III belongs to the antipodal series.

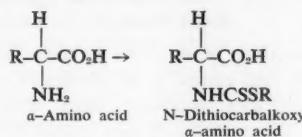
Various refinements of this approach have been introduced recently, including semiempirical generalizations that often make it possible to predict the sign of the Cotton effect of an unknown ketone without requiring the rotatory-dispersion curve of a reference compound. These generalizations have already been reviewed elsewhere (2, chaps. 9, 10, 13). Suffice it to say that by the approach illustrated in Fig. 7, or its extensions (2, chaps. 9, 10, 13), the absolute configurations of a wide variety of very complicated natural products have been established very quickly. A few typical examples are listed in Fig. 8, and it is quite obvious that to arrive at the same conclusions by any other classical approach would have required a tremendous effort.

Recent Studies

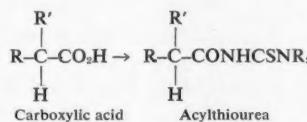
Most of the discussion up to this point has centered on the carbonyl chromophore, largely because most of our original work was focused on it by virtue of its abundance and its favorable spectral characteristics. Lately, our attention, as well as that of other investigators, has centered on other chromophores, and a very substantial amount of progress has already been achieved. Examples of chromophores recently studied are dithiocarbamates and xanthates (11), disulfides and diselenides (12), phthalimides (13), acylthioureas (14), and osmates (15). This partial list is important not only because it illustrates the range of different chromophores that may give rise to Cotton-effect curves but even more significantly because it includes derivatives of important nonchromophoric functional groups.

For instance, α -amino acids do not absorb in a convenient region of the ultraviolet, and thus they show only plain rotatory-dispersion curves, but their readily prepared N-dithiocarbalkoxy derivatives (dithiocarbamates) exhibit strong Cotton effects. In fact, the sign of the Cotton effect can be

related directly (11) to the absolute configuration of the asymmetric center, members of the L-series showing a positive Cotton effect and of the D-series, a negative one.



Similarly, carboxylic acids do not absorb above 2100 Å, but their derived acylthioureas (14) contain a low-intensity absorption maximum near 3400 Å. It is not surprising, therefore, that such thiourea derivatives should yield Cotton-effect curves, and that the sign of these Cotton effects can be used for deducing the absolute configuration of the α -asymmetric center of the parent carboxylic acid.



Olefins are essentially transparent above 2000 Å, but they react readily with osmium tetroxide to form osmate esters. These esters, notably their dipyradyl complexes, have been found (15) to exhibit strong Cotton effects in a very convenient region of the spec-

trum, and as these osmates can be prepared on a micro scale, there is now available a means of studying certain structural and stereochemical features of olefins in terms of anomalous optical rotatory-dispersion curves.

Conformation of Polypeptides and Proteins

One very exciting area of application of optical rotatory dispersion which has not yet been mentioned is represented by the determination of conformation of polypeptides and proteins. This has been reviewed recently in great detail by E. R. Blout (2, chap. 17), one of the experts in this field, and only brief reference will be made to it here. The hydrogen-bonded α -helix structure of proteins, initially proposed by Pauling and Corey, is now generally accepted. One of the most effective ways of studying such a conformational feature has been by optical rotatory dispersion, even though such curves have until recently been of the plain type. Specifically, it was found that the plain curves of polypeptides in a random conformation follow a simple Drude equation, while this is not true of the helical conformation, and the consequent mathematical generalization, largely attributable to W. Moffitt, has proved to be very useful in this biochemical area. As an illustration of the use to which this information can be put, Goodman and his collaborators (16) have demonstrated by such rotatory-dispersion measurements that the critical range where synthetic polypeptides of γ -methyl glutamic acid shift from the random to the helical structure falls between the hepta- and the nonapeptide.

Two important recent observations by Blout and his collaborators indicate that anomalous optical rotatory dispersion with experimentally determined Cotton effects will play an increasingly significant role in the polypeptide and protein fields. In the first paper (17) it was noted that while poly- α , L-glutamic acid exhibits only a plain rotatory-dispersion curve (A in Fig. 9), its complex with the optically inactive dye acriflavine now shows a Cotton effect (B in Fig. 9) centered around the 4570-Å absorption band of the dye. Such Cotton effects are observed with the dye-polypeptide complex only when the polypeptide is in the helical, rather than the random, conformation. The com-

plexes thus offer a strikingly simple approach to the study of helical conformations as well as to the determination of the "handedness" of the helix by means of anomalous rotatory dispersion. Of even more general applicability is the observation (18) that Cotton effects of polypeptides and proteins may be determined directly in the 2100- to 2300-Å region with the new automatically recording Rudolph spectropolarimeter, and this development points toward the most significant contribution that improved instrumentation can make.

Future Prospects

As spectropolarimeters are designed that can either handle strongly absorbing systems or else permit greater penetration into the ultraviolet region, more and more classes of organic compounds will fall within the scope of the type of anomalous optical rotatory-dispersion analyses that have been covered in this article—analyses that have been responsible for the suddenly revived interest in optical rotatory dispersion and its widespread use in organic chemical and biochemical research problems.

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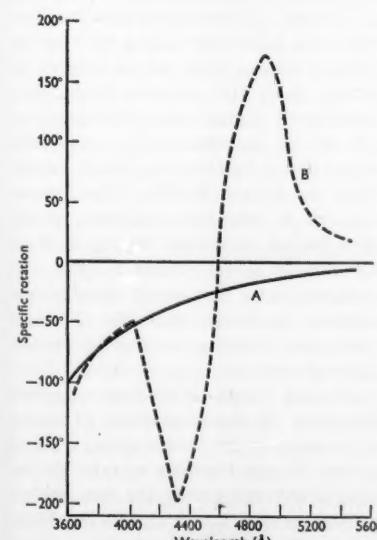


Fig. 9. Optical rotatory-dispersion curve of poly- α , L-glutamic acid at pH 4.9. A, Polypeptide without dye; B, polypeptide-acriflavine complex. [Redrawn from Blout and Stryer (17) with permission.]

Science and the News

End of the Test Ban: The White House Response Is Restrained; Educational TV in Court

Last week, after the 338th session of the Conference on the Discontinuance of Nuclear Testing, Sir David Ormsby-Gore, the chief British delegate, told reporters that he was convinced that not only had the Russians no intention of negotiating seriously for a test-ban treaty, but that their loss of interest was so complete that he doubted they would go through with a treaty even if the West were to give in on every point in dispute. Within a few hours the Russians announced they would resume testing in terms which indicated that Ormsby-Gore's intuition was perfectly correct, for the Soviet announcement implied a resumption of testing of high-yield weapons in the atmosphere, and this is, indeed, the kind of test the Russians have carried out. Such tests, of course, are impossible to conceal, and the Russians could not have carried them out while any test-ban agreement was in force, even one which, like the *de facto* ban that has existed for the past 3 years, provided no formal controls at all.

The White House first learned of the Soviet intentions while Kennedy was at a press conference, indeed almost at the moment when, in answer to a question, he was stating that Ambassador Dean would be brought home from Geneva at the end of this week, at which time, barring an unexpected change in the Soviet attitude, the Administration would be expected to make the "appropriate decisions." This was not news, merely a reiteration in response to a question, of the position the President had taken 2 weeks earlier. There was, therefore, considerable surprise at the Soviet action: for although it was generally assumed that the Russians wanted to resume testing, it was also assumed that the Russians would wait for the United States to act first, rather than take the onus of breaking the ban themselves.

The official White House reaction to

the Russian announcement is that "what the Soviet Union is obviously testing is not only nuclear devices but the will and determination of the free world to resist such tactics." This view is widely accepted, and indeed is the only one that has been offered that seems to explain the timing of the Russian move. In this view, the Russians have adopted tactics quite similar to those used by Hitler: to make aggressive demands in piecemeal fashion, so that no single instance involves such a clearly unreasonable concession that the noncommunist countries would be united in the view that a firm stand is necessary, even at the risk of war; simultaneously the aggressor both insists on his desire for peace and brandishes his military power and his willingness to use it, so encouraging the view that it would be better to give in and have peace, rather than to risk war on an issue, such as Berlin, which, by itself, may not seem important enough to fight over. The difficulty, of course, is that to give in not only does not avoid war in the long run; it also encourages the aggressive power to use such tactics even more freely in future crises and at the same time demoralizes the opposition to aggression.

The generally accepted view is that the Russians do not want war, indeed that they cannot rationally want war, but that they want as much as they can get short of war, including whatever can be gotten through threats of war. This is not a pleasant situation for anyone, least of all the President, on whom the brunt of the burden falls. His appearance at recent meetings with the press has reflected the strain he is under, as have his somber replies to questions, particularly at a recent off-the-record appearance before a State Department briefing for reporters and editors from around the country. But there seems to be no alternative but to stand firm on essential issues, such as access rights from West Germany to West Berlin, even while stating a readiness to negotiate issues that are negotiable. In this way, it is hoped, the Rus-

sians can be shown that there is nothing to be gained by threats of war, and a good deal to be lost by actions which both increase the chance of a mutually disastrous war and dissipate the considerable sympathy Russia now has among the uncommitted nations.

Soviet Strategy

Essentially what seems to have happened, and seems to be confirmed by Khrushchev's comment that, among other things, what Russia wants from the Berlin crisis is recognition of its "grandeur," is that the Russians feel that their achievements in space, their missiles, the general shift of the balance of military power, entitle them to demand concessions from the West in recognition of their new power. The West recognizes a shift in the military balance toward parity between the United States and Russia, and that this inevitably strengthens the Russians' influence in international affairs. The Administration's expanded defense program is designed to see that the balance gets no worse than parity. The immediate problem of the West is to demonstrate to the Russians that their relative increase in military strength still has not given them the power to demand concessions by threat of force, and to try to rally wavering allies and the neutrals to the idea that it is in the interest of all noncommunist countries to support a firm stand against Russian demands for concessions, based on threats of a nuclear holocaust.

Without an assumption that the Russians are consciously using the resumption of testing, with the accompanying boasts about 100 megaton bombs, as a weapon of intimidation, the timing of the Soviet announcement makes little sense. For it had become almost certain that the United States, if the Soviets showed a little more patience, would feel forced to resume testing itself in the face of Soviet refusal to agree to a control system that would provide reasonable assurance that the Russians were not secretly conducting underground tests.

Several things, aside from apparent desire to use the resumption of testing as a tactic in the Berlin crisis, made it easier for the Russians to take the responsibility for ending the ban, and to a large extent they center on the desire of the Russians to develop very large bombs. Khrushchev had made explicit in talks with Westerners in recent months the view that the Russians have a strong interest only in testing very

large weapons, and such tests would be very difficult and expensive to conduct underground. To the extent that the value of a 100-megaton weapon is largely psychological, as an instrument of intimidation, there would, in fact, be a desire to make a show of the explosion.

But the United States had given ample indication that, if it resumed testing, the tests would be conducted underground. This meant that the choice before the Russians was not between taking the blame for resuming testing and waiting awhile for the U.S. to take the blame, but the narrower choice of starting atmospheric testing before or after the U.S. had announced that it would resume underground tests. This option was made narrower still by the modest but noticeable degree of U.S. success in its efforts to convince the world that it was the Russians who were blocking the test-ban treaty; that there could be no assurance, in the absence of a control system, that the Russians were not conducting secret underground tests; and that therefore, as a result of Soviet intransigence, American resumption of underground testing was becoming unavoidable.

Appropriate Decisions

The "appropriate decisions" Kennedy had promised if a break in the Geneva deadlock failed to develop were not expected to include an immediate announcement of testing, underground or otherwise. The immediate announcement on this point was expected to go no further than to state that we would have to resume underground tests fairly soon in the absence of a change in Russian mood. This would be a shade stronger than previous announcements merely restating the fact that we could not accept the unpoliced ban indefinitely but without stressing that a decision was near. As was suggested by Kennedy's reference to "decisions" rather than "a decision," the announcement, however phrased, would not have been limited to talk of resuming testing; indeed it apparently would have included an offer to sign a treaty barring at least atmospheric tests, the tests which are of the most concern because of the fallout hazard and for which a ban can be effectively policed even without a formal control system. And again, the announcement was likely to include an offer to submit the whole question of what would constitute an adequate control system to a commission appointed by the U.N.

No one in the Administration thought that all of this would lead the world to welcome an American resumption of underground testing, but there were indications that the elaborate display of Western patience at Geneva, including several modest, but new, concessions made as late as last week, was winning some sympathy for the Western position. As noted, this made it somewhat less tempting for the Russians to delay taking the decisive move first, in the hope of reaping a propaganda advantage by forcing the U.S. to break the ban. In a sense, the result has been a victory for the Administration and its insistence on following a patient policy rather than rushing into testing as soon as it became clear the Russians did not intend to agree to a meaningful treaty. But it is a victory with such depressing consequences that no one is particularly anxious to celebrate it. The most that can be said is that things would have been just so much worse if the Russians had been able to point to American underground testing as the excuse for atmospheric testing.

Meanwhile, on the question of disarmament and the test ban, the Administration has been sticking to its policy of "walking the extra mile" to keep open the hope for agreement. In this field propaganda and genuine interests are so deeply intertwined that it is rarely possible to separate the two. There is a good deal of both in Kennedy's announcement that the need for a new U.S. Disarmament Agency, now before Congress, is more urgent than ever; in the lack of any hurry for the U.S. to make the inevitable announcement that we, too, must resume testing; and in the continuing plan to press the whole issue at the U.N. But given the current Russian mood, it appears futile to look for results, for the present, anywhere but in the realm of propaganda. —H.M.

Educational Television in Court

Noncommercial, educational television, which has developed a small, nationwide network unaffected by audience ratings or advertisers' interests, sought a legal remedy last week for Federal Communications Commission policies that give commercial stations priority for the few vacancies left on the very-high-frequency band. The VHF band is the sole access to most of the nation's TV sets.

The occasion for legal action was

FCC decisions favoring commercial operation of new channels sought by Pennsylvania State University and the Rochester (N.Y.) Area Educational Television Association.

In a petition filed with the Court of Appeals in Washington, the Joint Council on Educational Broadcasting, representing a number of major educational organizations, joined with the Rochester group in an attempt to block the FCC's Rochester decision, which was a final one. Simultaneously, Pennsylvania State University petitioned the court to direct the FCC to reconsider a preliminary decision which favors establishment of a new commercial outlet at Johnstown, Pa. In each area, it was pointed out, there are now two VHF commercial stations in operation, and decisions favoring establishment of a third do not conform with the FCC's avowed policy of encouraging the growth of educational TV.

The impetus to seek relief from the court came from a number of factors which have enhanced educational TV's potential outside the classroom and placed its growth over the next few years directly against a substantial barrier. The barrier is founded on the view of a majority of the Commissioners that, although educational TV merits the official blessings with which it has been showered, its growth on VHF must be subordinated to the fostering of competition between commercial stations. Accompanying this view is the assumption that competition between commercial television organizations is more likely to produce quality than is competition between commercial and noncommercial television.

In the long run, there will be plenty of room for commercial as well as noncommercial television, for the only direction for substantial expansion lies on the ultra-high-frequency band (470 to 890 Mcy/sec), which provides 70 channels, in contrast to the nearly saturated VHF band (54 to 216 Mcy/sec), which contains 12.

Increased availability of channels that can be widely received could lead to programming, such as has developed on FM radio, aimed at segments of the audience uninterested in mass entertainment. But until there is a break in a circle made up of limited UHF broadcasting because of few UHF receivers, and few UHF receivers because of little UHF broadcasting to receive, the question arises of who gets the few remaining outlets that reach the bulk of existing TV

sets. These outlets come into being usually when the FCC agrees, as it did in the Rochester and Johnstown cases, to open new channels on the VHF band by permitting substandard spacing. While educational broadcasters are pleased with the long-range prospects of ample space on the UHF band, they feel that FCC policies require them to enlist their limited resources in the FCC's program of breaking the UHF circle.

The goal of expanded UHF broadcasting is being pursued by the FCC in a number of ways, including a proposed program of "de-intermixture," which is intended to bring all-UHF broadcasting to communities where UHF is now broadcast, usually at a great disadvantage, in competition with VHF. In addition, the FCC has asked Congress to require manufacturers to equip new sets for UHF, a proposal that has received little congressional support, while the "de-intermixture" proposal has stirred protests from various congressmen and from at least one complete state delegation.

Congressmen from rural districts find VHF, with its generally longer range, useful for reaching their rural constituents and are concerned about how long it would be before UHF facilities would be available for this purpose. Some of the protesters are not well informed about the problem and have not looked into the availability of answers, but, finding the present arrangement satisfactory, are reluctant to change. In addition, the existing VHF stations in areas where the band is filled are not receptive to moving to a band that would have room for competition.

Underlying the court challenge to FCC's inhospitality to educational TV's growth on the VHF band is the fact that educational TV is successful on most of its active outlets (the bulk of these are VHF) and that its prospects for increased financial support are now greatly heightened by widespread revulsion toward what comes over the commercial channels.

Operating as a "fourth network," some 50 educational TV stations, supported largely by private grants, contributions, and state aid, are affiliated through the National Educational Television and Radio Center. By swapping films, tapes and pooling resources, they have maintained a high level of public service and have frequently presented distinguished fare. This "fourth net-

work," if it could expand to additional channels that reach the public, could fulfill Walter Lippmann's suggestion that the answer to the ills of TV "is to devise a way by which one network can be run as a public service with its criterion not what will be most popular but what is good."

A bill to provide each state with Federal assistance for the construction and improvement of educational TV facilities has been approved by the Senate each year for the past three years. While it has been cleared by committee it has never come to a vote in the House but its prospects for next year are considered bright. This, too, has provided pressure for removing FCC obstacles to VHF channels for educational TV.

The maintenance of the obstacles cannot to any substantial extent be blamed on the commercial TV industry, for, if nothing else, when the question arises of who is to get a new community outlet, commercial or noncommercial interests, the existing commercial stations are not displeased to see it go to an organization that will not compete for advertising. One of the major networks, for example, came out strongly at House hearings for federal aid to educational TV.

The principal source of the obstacles blocking educational TV's expansion on the VHF band is in the extreme conservatism of the present majority of FCC commissioners, who are firmly tied to inflexible notions on the issue of public versus private operations on the airways. With the absence of substantial congressional interest in the future of educational television, the FCC's present majority casts doubt on the accuracy of Chairman Newton Minow's forecast that ". . . if there is not a nationwide educational television system in this country, it will not be the fault of the FCC."

The plaintiffs in the two court actions, pleased by Minow's support but doubtful of his prophecy, are seeking a way around the FCC.—D.S.G.

Salt-free Water

The President will shortly sign a bill providing long-term authorization for a saline water conversion program. Both House and Senate have passed bills authorizing \$10 million a year for this

project, about 6 times as much annually as is now being spent. Even at \$10 million a year, the program is relatively small as major federal research and development programs go, but it has attracted a good deal of attention. An economical method of converting sea water to fresh water would be immensely useful for this country, which faces a water shortage in the decades ahead, and would revolutionize the economies of a good many countries which have always faced chronic water shortages. The most efficient plants now available can process salt water at a cost of about \$1 per thousand gallons, but this is still 3 times too much for drinking water, and nearly 10 times too much for irrigation water. Kennedy has drawn attention to the project in several speeches, and he sent a message to Congress requesting the expanded program.

Foreign Aid Appropriations

The House Appropriations Committee demonstrated last week that Administration leaders had been unduly optimistic when they assumed that the newly passed foreign aid bill would impose upon the committee a moral obligation to reduce its traditional antagonism to foreign aid spending.

Even before the bill had passed both houses, an appropriations subcommittee headed by Rep. Otto E. Passman, of Louisiana, had made substantial cuts in the accompanying money bill. In backing up Passman, who is openly hostile to foreign aid, the full committee illustrated once again why the Administration wanted long-term foreign aid and the financing of this aid through Treasury borrowing. The basis of the Administration's argument for this method was fear that the Appropriations Committee would behave in just the fashion that it did.

The reductions voted by the committee, which sent despair and astonishment through the Administration, consisted of \$896 million from the \$4.2 billion foreign aid bill, plus refusal to permit the Administration to use \$96 million carried over from the last fiscal year.

In the annual melodrama of foreign aid appropriations, the script calls for a great deal of this to be restored on the floor of the House, but just how much will be restored is, of course, never certain; nor is this system conducive to the long-range planning which

the Administration regards as essential for successful development programs in underdeveloped nations.

The assumption of a moral obligation upon the committee was voiced by Sen. Fulbright, among others. Having led the President's unsuccessful fight for long-term Treasury financing of foreign aid, which would have eliminated the need to run the Appropriations Committee gauntlet, Fulbright said the compromise of a 5-year authorization and annual appropriations was acceptable. The White House termed it "wholly satisfactory," and suggested that the 5-year authorization carried with it assurances that the money would be available.

House Appropriations, however, has made it clear that it remains the judge of its moral obligations.

School's Out

Once again last week, the Administration's public school program was reported to have been killed. Such reports have been appearing regularly all summer long, but the latest was quite accurate: the school bill is now really dead. The blow came when an attempt was made to bring up an emasculated version of the President's recommendation, using the peculiar "Calendar Wednesday" procedure to bypass the Rules Committee. The effort to consider the bill under the Calender Wednesday procedure failed, indeed failed miserably to muster the necessary majority. The vote was 242 to 169 against consideration, despite the modest nature of the bill, which asked only for a classroom construction program not much different from that contained in a bill passed by the House last year; it included no money for teachers' salaries.

The vote came early in the afternoon on the day of Kennedy's press conference last week. At the conference Kennedy, in effect, conceded defeat by answering a question about the vote by promising that the Administration would try again next year. There was still some hope that a version of the Administration's program for higher education would get through the House, but whether it does or not can only decide how complete the Administration's defeat on education will be. Nothing that is at all likely to emerge now can add up to more than a shadow of the fairly ambitious program Kennedy had recommended.

Announcements

A "bureau of standards" for heart disease control tests has been introduced at the U.S. Public Health Service's Communicable Disease Center, Atlanta, Ga., which will provide a central depot of preserved samples of human serum previously analyzed for their cholesterol content. These will be supplied to laboratories across the country, to be used as a standard against which the laboratory can check its own procedure. Heretofore, because of differences in cholesterol-measurement standards, analyses of the same blood sample by different laboratories (and sometimes even by the same lab) often produced varied results. Pooling of data from different studies was also made difficult by this variance. Anticoagulants and clot-dissolving agents are due for standardization efforts in the near future.

The United States has invited weather scientists from more than 100 countries, including Russia, to attend the International Meteorological Satellite Workshop to be held in Washington, D.C., from 13 to 22 November. The U.S. Weather Bureau and the National Aeronautics and Space Administration have sent invitations to all countries belonging to the World Meteorological Organization.

Information about rare forms of mental retardation is available through the Rare Case Registry maintained by the Southern Regional Education Board. The service, available to research personnel employed in academic or clinical facilities anywhere in the 15 Southern states, will supply researchers with a list of institutions which have cases of the type requested, and with the names of officials to be contacted. Requests, to be submitted by letter, should include place of employment and a specific description of the kind and number of cases needed. (Rare Case Registry, Southern Regional Education Board, 130 6th St., NW, Atlanta 13, Ga.)

A Soviet study of rare-earth spectroscopy has been translated and released by the U.S. Atomic Energy Commission. The two-volume monograph, published in 1953, is described by Russian scientists as the first systematized analysis of the experimental and theoretical

data in this field. (Office of Technical Services, U.S. Department of Commerce, Washington 25, D.C. Order No. 61-31483. \$7)

Courses

A university credit course on new trends in biology will be featured on CBS's "College of the Air" TV series, beginning 25 September. The 32-week course, developed by CBS and the Learning Resources Institute of New York, in consultation with the American Institute of Biological Sciences, will be directed by Ray Koppelman of the University of Chicago. There will be five 25-minute lecture-demonstrations per week, the time of presentation to be determined by the individual station. More than 100 colleges and universities will give credit to those who fulfill the course requirements. [Thomas P. Robinson, Learning Resources Institute, 680 5th Ave., New York 19 (program administration); Ray Koppelman, Department of Biology, University of Chicago, Chicago, Ill. (course content)]

A 2-year graduate program in radiologic physics is being offered at the Stanford University School of Medicine. The program, open to students with a B.A. or a B.S. degree in physics, leads to an M.A. degree in the medical sciences, with specialization in medical or health radiological physics. A limited number of fellowships will be available to qualified students. A 1-year traineeship in medical radiological physics is also available. (Chairman, Department of Radiology, Stanford Medical Center, Palo Alto, Calif.)

Meeting Notes

Meteorologists in over 100 nations have been invited to attend the International Meteorological Satellite Workshop to be held in Washington, D.C., from 13 to 22 November. The workshop is being arranged by the U.S. Weather Bureau and the National Aeronautics and Space Administration, with the cooperation of scientific and international groups. Participants will prepare weather analyses from satellite photographs to gain practical experience in the use of satellite data distributed by the U.S. through international meteorological channels.

The annual meeting of the Society for the Scientific Study of Religion will be held in Cambridge, Mass., on 27 and 28 October. The meeting will include a panel discussion on Eskimo, Hindu, and Jewish conceptions of man's place in the universe, and reports on anthropological, sociological, and psychological empirical studies. (James E. Dittes, SSSR, 409 Prospect St., New Haven 11, Conn.)

A study-conference on the role of biomedical engineering in universities and hospitals will be held on 26 and 27 October in Omaha, Nebraska. (Harold G. Beenken, University of Nebraska College of Medicine, 42nd St. and Dewey Ave., Omaha 5)

Scientists in the News

William B. Lurie, physicist and former senior project engineer with General Precision Laboratories, has been appointed chief engineer, Burnell and Company, Pelham Manor, N.Y.

Harley C. Shands, associate professor of psychiatry at the University of North Carolina Medical School, has been appointed professor of psychiatry at the State University of New York Downstate Medical Center.

Wallace Windus, chemist with the U.S. Department of Agriculture, has received the 1961 Alsop award of the American Leather Chemists Association.

Paul Silva, former professor of botany at the University of Illinois, has been named senior herbarium botanist at the University of California, Berkeley.

Edmund S. Nasset, professor of physiology at the University of Rochester Medical Center, will spend an 18-month sabbatical leave as nutrition adviser to the government of India, under the United Nations Food and Agriculture Organization.

Monroe E. Freeman, deputy assistant director for general research of the Advanced Research Project Agency, Department of Defense, has been appointed director of the Smithsonian Institution's Science Information Exchange. He succeeds **Stella L. Deignan**, who recently resigned.

Albert Schatz, chief of the division of microbiology at the Philadelphia General Hospital and associate in medicine at the University of Pennsylvania Graduate School of Medicine, has received the \$4000 Dental Research prize of the Soil and Health Foundation of Emmaus, Pa.

James A. Fancher, biochemist and former research associate with the Upjohn Company, Kalamazoo, Mich., has been appointed an assistant member of the research laboratories of the Albert Einstein Medical Center, Philadelphia, Pa.

John Higginson, professor of pathology at the University of Kansas School of Medicine, has been named American Cancer Society Professor, in recognition of his work in cancer epidemiology. He will head the university's newly created department of geographical pathology.

Finn J. Larsen, physicist and former consultant for research and development to the Secretary of the Army, has been named Assistant Secretary of the Army for Research and Development.

Franklin D. Yoder, director of the Division of Environmental Medicine of the American Medical Association, has been appointed Director of Public Health of the state of Illinois.

Charles V. Theis, staff scientist in the U.S. Geological Survey's water resources division, has been given leave from the Survey to head Columbia University's new program in hydrogeology, as adjunct professor in geology.

Martin I. Blake, associate professor of pharmacy at the University of Illinois, has been named professor and head of the department of pharmacy in the university's College of Pharmacy.

Conrad W. de Fiebre, senior research bacteriologist with E. I. du Pont de Nemours, has been appointed research director of the Wilson Laboratories, the pharmaceutical division of Wilson and Company, Chicago.

George E. Dombrowski, former senior engineer with Raytheon Manufacturing Company, Waltham, Mass., has been appointed associate professor of electrical engineering at the University of Connecticut School of Engineering.

Recent staff appointments in the U.S. Atomic Energy Commission's Division of Biology and Medicine:

William W. Burr, Jr., biochemist, formerly of Southwestern Medical School, has been named chief of the division's medical research branch.

Richard S. Caldecott, geneticist with the U.S. Department of Agriculture at the University of Minnesota, has joined the division's headquarters staff.

Arthur T. Thompson has been named associate dean of the Pennsylvania State University's College of Engineering and Architecture. He was formerly associate director of the Ordnance Research Laboratory, operated by the university for the Navy Bureau of Weapons.

Recent Deaths

Abbe H. Breuil, 84; paleontologist; taught at the University of Fribourg, Switzerland, at the Paleontological Institute of the College of France, and at the universities of Lisbon and Johannesburg; 14 Aug.

Percy W. Bridgman, 79; physicist and 1946 Nobel Prize winner; retired Harvard faculty member; 20 Aug.

Oswald Frommelt, 88; former horticulturist with the U.S. Department of Agriculture; 14 Aug.

William C. Gibson, 46; associate dean of the University of Michigan School of Public Health; 17 Aug.

Gladys M. Keener, 64; assistant to the executive director of the Scientific Manpower Council and former executive editor of *Science*; 19 Aug.

Cyrus C. MacDuffee, 66; chairman of the University of Wisconsin's mathematics department until 1956, and former secretary of the mathematics section of the AAAS; 21 Aug.

Ora S. Morgan, 84; emeritus professor of agricultural economics at Columbia University; 14 Aug.

William K. Sproule, 48; consulting metallurgist with the Development and Research Division of the International Nickel Company, New York; 8 Aug.

Gotthold Steiner, 75; chief nematologist for the U.S. Department of Agriculture until his retirement in 1955; 21 Aug.

George E. Tate, 56; nuclear physicist with the Foster Wheeler Corp., New York City, since 1930; 24 Aug.

Max M. Van Sandt, 58; medical director of the U.S. Public Health Service, Washington, D.C.; 22 Apr.

Book Reviews

A-Bomb Story—British View

The Birth of the Bomb. The untold story of Britain's part in the weapon that changed the world. Ronald W. Clark. Preface by Sir George Thompson. Horizon Press, New York; Phoenix House, London, 1961. 209 pp. \$3.95.

Subtitled "The untold story of Britain's part in the weapon that changed the world," this book is a long-needed and welcome filler for a lamentable gap in the published history of nuclear weapons and atomic power. Done with meticulous and detailed care and a scholarly thoroughness and with an almost painful effort to be truly fair to both British and American sides of the story up to Hiroshima and Nagasaki, the account is much more personal and circumstantial than its American counterpart, the "Smyth Report." The latter was devoted largely to chronology, basic physics, and engineering detail, while this one retells romantic happenings and gives vignettes of personalities and of the sometimes ironical circumstances attending the strictly physical developments. The book follows by 16 years the White Paper, "Statements Relating to the Atomic Bomb," put out by the British Government in 1946, immediately after the publication of the American report. Unfortunately Clark neglects to mention that the Smyth Report was not merely a matter of "natural American exuberance" and pride of accomplishment but that it was also made with the serious purpose of facilitating ultimate international control of atomic energy, the need for which was clearly seen by many. Only by such publication could there be any hope at all of such control: a hope now, alas, belied.

The author has skillfully conveyed the sometimes amazingly informal atmosphere in which much of the British research during the years 1939 to 1941

was conducted, and he brings out the irony of the situations in which extensive nuclear research was conducted by non-British subjects, who, being non-British, were thereby debarred from helping with radar, magnetic mines, or other classified work. In this country also, how to keep random scientists from thinking classified thoughts remained a puzzle to the security personnel throughout the life of the Manhattan District.

The early French contributions to the data on nuclear fission and the existence of French patents—taken out in 1939—on bombs and piles are of course no secret, and the romantic story of heavy water has been fully publicized. But the author is surely right in saying that the general public is not aware of the vital role British research and scientific thought played in starting the United States off aright, even though Clark quotes President Truman as saying, "Britain, whose scientists had initiated the project and were contributing much of the original atomic data . . . , and Arthur Compton as saying, "The British approach was, from the beginning, however, focused specifically on a definite weapon whose construction was roughly outlined. . . . In 1940 it was still difficult for us in America to concentrate our thoughts on war, while for the British it was their prime concern . . ." (to pick two representative informed samples). The scientific public can be glad that the present book is so full and so informative.

The U.S. Decision

It seems that "the Americans suddenly found themselves in the Fall of 1941 standing unexpectedly on the threshold of the nuclear age" and really got started on it with the third report from the National Academy of Sciences. To some extent that decision to start was independent of the British, since the "Maud Report" was received

"under terms which did not permit disclosure to the Academy Committee." But with the return of Pegram and Urey from Britain with full information on the British progress, there was no room for doubt as to the bomb's ultimate feasibility. The author says, "It would be as wrong to underestimate [the American] creative work in building the bomb as it would be to ignore the pioneer work of the British in cutting through the mental undergrowth that first had to be cleared away." Not merely clarified thinking but good solid experimental data, calculations based on them, and actual techniques for making barriers and other things were to be had from Britain. Typical of the author's care in passing judgment is: "The bomb would probably have been made had Chadwick and Peierls and Frisch and Thomson never bent their energies and ingenuity to the task; but it would probably have been too late for this particular war." Nationalistic pride in the scientific accomplishments of one's own country seems to be a more vigorous emotion abroad than that it is here, or at least it is more often voiced, perhaps because so many of our best brains are recent additions to our nation. This book, too, is instinct with a quite proper pride in the extensive contributions made by the British. But it is faintly ironic that in its index among the 36 scientists listed and actively connected with the bomb in England, 14 names are of immigrants and 22 are of native Britons. And contrasting with this proper pride is just a touch of the feeling expressed in the comic English history *1066 and All That*, whose last sentence reads, "America Thus became Top Nation, and History came to a [full stop]."

Morality and the Bomb

Historical material occupies the first 11 chapters of Clark's book, but in the last one, "The end product," the author's own views on war and morality get full play. Everyone must decide for himself as to the morality of the use of the bombs at Hiroshima and Nagasaki, but to me such armchair exercises seem futile in the face of the hideous immorality of war itself. However, to one phrase of the author's, more vigorous exception must be taken: it was no "stumbling decision to drop the first nuclear weapons," but a decision long and most seriously examined in all its implications, with the result that even the scientific advisers of the committee

set up by President Truman could "see no alternative to direct military use." I suggest that Clark should reread page 419 of President Truman's *Year of Decision*. There had been a rejection of the Potsdam ultimatum, and there had been no word at all from the Japanese after Hiroshima. Nagasaki was the necessary clincher. I submit it is unnecessary to "speculate which of many factors was uppermost in American minds in the Summer of 1945." The only mind that mattered here was Truman's, and he has given his reasons *in extenso*. To give the author his due, he does balance the various possible motives, irrelevant though they may be, but to imply that Nagasaki was merely a test firing of a plutonium weapon is to forget too much what war is like.

This criticism, of course, has no relation to the value of the book as a significant contribution to recorded history.

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Cultural Internalization

Structure and Personality. A casebook.

Yehudi A. Cohen. Holt, Rinehart and Winston, New York, 1961. xiv + 528 pp. \$7.50.

Much that we call personality represents the internalization of the culture into which one is born. The emergence of "culture and personality" as a field of study, in the decade before World War II, helped to break down narrow disciplinary parochialism in psychology, sociology, and anthropology. In its early form, however, the culture and personality approach tended to exhibit two types of oversimplification: (i) treatment of the culture as though it were a relatively homogeneous affair, influencing all members of the society in much the same way and (ii) a tendency to see the child as an overly plastic, almost passive recipient of cultural shaping. This volume by an anthropologist with a social psychological perspective draws heavily on studies of nonliterate cultures and on cross-cultural studies, but the author-editor very largely avoids the pitfalls of too easy generalization from "simpler societies."

No person participates in the whole of his culture, even in the simplest societies. The cultural content and interpersonal expectations which one internalizes depend upon one's position

within the social structure—the network of relationships between individuals, families, and other groups making up the society. Moreover, socialization is always oriented toward preparing the child to live in the kind of world that the parents anticipate, which may not be the world as it now is. These axioms guide the author in examining the effects of societal institutions on the socialization of children and on the course of growing up, in considering aspects of personality which are required for the successful functioning of particular institutions, and in noting the effects of institutions on the personality processes of adults. Drawing upon recent research on social structure and socialization, Cohen documents the ways that race, social class, and changing values influence the content that parents transmit to their children and the ways that parents exercise control over children.

If different positions in the social structure tend to produce different emphases in personality organization, one must inevitably be concerned with the fit between these personality constellations and the requirements of various adult roles and institutional arrangements. The case materials drawn on for this analysis relate to occupations, military and caste structures, and modal patterns of total social systems. Another problem of fit occurs when people move from one social system to another or when the social system undergoes marked change in a relatively short period. Finally, in a section on the sociological conditions of personal disorganization, Cohen examines the ways in which psychosis is manifest in different cultures and in different strata within our own culture and develops briefly the view that psychosis must be seen as one aspect of the individual's placement in his social structure.

While called a casebook, this is no mere collection of readings from the literature. It is a well-reasoned and well-organized presentation of the relationships between social structure and personality as revealed primarily by anthropological studies, but with some inclusion of sociological and psychological research as well. This book does not wholly avoid the impression that man is plastic to be molded; it does achieve a coherence and cogency that will certainly lead to its wide use by social scientists interested in personality.

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Marriage of Convenience

Retrieval Guide to Thermophysical Properties Research Literature. vol. I. Book 1, pt. A, *Guide to Substance Classification and Numerical Codes*. pt. B, *Dictionary of Synonyms and Trade Names*. pt. C, *Directory of Substances*. xii + 11 pp. + 90 pp. + 1250 pp. Book 2, *Classified Search Index*. vii + 169 pp. Book 3, *Master Bibliography. Author Index*. ix + 372 pp. Y. S. Touloukian, Ed. McGraw-Hill, New York, 1960. \$120.

The growing number of information centers which selectively condense reference material in specialized fields is indicative of the literature ills afflicting research today. More than 400 such centers presently exist in the United States, and the trend is to be encouraged, if worthwhile publications result. A leading example of these centers is the Thermophysical Properties Research Center (Purdue University) which has cooperated with McGraw-Hill Book Company in the publication of the *Retrieval Guide to Thermophysical Properties Research Literature*. This machine-generated reference tool reduces an extensive literature search to a unique, systematized, "look-up" procedure, the end product of which is a reference to a publication containing the detailed information.

Volume I, now published, covers seven thermodynamic and transport properties (thermal conductivity, specific heat, viscosity, emissivity, diffusion coefficient, thermal diffusivity, and Prandtl number) for 14,240 substances. Two proposed volumes, to be published at intervals of 12 to 18 months, will survey the literature from about 1920 onward for nearly 30 such properties (coefficients of thermal expansion, surface tension, vapor pressure and P.V.T. data, derived thermodynamic properties, latent heats of fusion and vaporization, critical constants, boiling and melting points, thermoelectric constants, and electrical conductivity). The three books of volume I (a desk-size counterpart of computer records) provide comprehensive, though not easy, access to book, journal, and report literature on data, theoretical approaches, and experimental measurement techniques.

In book 1 the user can find, arranged by either formula or name, the material of interest, its substance number, and a listing of the properties re-

ported. Synonyms can be traced in the "Dictionary of synonyms and trade names." If the property is listed in book 1, the user then locates his substance number within the appropriate property division of book 2. Simple coding of the physical state of the substance and the subject slant, language, and year of publication of the article enables the searcher to make a knowledgeable selection of the literature cited. The serial number of the citation chosen is then traced among the 10,000 items listed in the "Master bibliography" of book 3. These bibliographic entries point the searcher to the primary literature where the desired information has been published. The preliminary pages of each book present much descriptive and informative material, the importance of which cannot be overemphasized. The summaries located inside the back cover of book 1 will be praised by all those who use the set repeatedly.

The major hurdle in using the volumes is the identification of the substance code number. A work which covers all matter must, of necessity, have a complicated classification system, but I feel that small inconsistencies in the arrangement of the "Directory of substances" (book 1) are unnecessary aggravations which reduce retrieval efficiency. The segregation of classifications 100 and 200 into separate alphabets and the alphabetization of individual classes in the higher series is not immediately evident, and this nonuniformity may not always be remembered by searchers. Occasional users and the uninitiated may, therefore, misuse the "Alphabetized name directory." The value of a single alphabetical arrangement, including the "Dictionary of synonyms and trade names," should be given careful consideration. One could then postpone decisions on classification until the various coding possibilities became evident under the name of the material.

Other criticisms concern format. Thumb indexing books 1 and 2 would speed access to particular sections. Additional properties to be covered in later volumes should be listed on the spines. Improved typefaces now available through computer-controlled photocomposition would make books 2 and 3 easier to use.

The high cost of the set may preclude its purchase by many individuals and force the use of library copies; therefore, research libraries will find purchase a necessity. The documentation group

at the Center hopes to gain some financial support from this limited market.

At present, machines do not solve the information retrieval problem, but computer-generated volumes such as the *Retrieval Guide* present a marriage of machine and book which offers one solution. For example, anyone who contemplates a literature search on the effect of dichloroacetic acid on the viscosity of polystyrene or on the specific heat of oxygen fluoride and who also realizes that in 10 minutes the *Retrieval Guide* can provide the answer must appreciate the magnitude of this work.

RICHARD L. SNYDER

Science Library,
Massachusetts Institute of Technology

Borrowed Behavior

Perspectives in American Indian Culture Change. Edward H. Spicer, Ed. University of Chicago Press, Chicago, Ill., 1961. x + 549 pp. Illus. \$10.

Anthropologists have written many case studies describing what happens when different cultures meet, but it has not been clear how these complex descriptions could be compared to yield further general understanding of culture change. This volume represents a major advance in the comparative analysis of change.

The volume is the outcome of an interuniversity summer research seminar (held in 1956) on the comparative study of acculturation. The seminar, sponsored by the Social Science Research Council, brought together six anthropologists who are engaged in the study of North American Indian groups: the Yaqui (Spicer), Rio Grande Pueblos (Dozier), Mandan (Bruner), Navaho (Vogt), Wasco-Wishram (French), and Kwakiutl (Codere). The editor introduces and summarizes the seminar's general approach and findings, while each author presents his case analysis.

The distinctive features of the seminar's approach are the particular kind of regularity in culture change which it sought to elucidate and the techniques which it developed for this purpose. Faced with a fascinating diversity of responses to culture contact, the seminar's participants found it most fruitful to seek regular relationships between types of culture change and types of contact situations. To do this, the history of each culture was first divided into a

series of "periods" which appeared to be relatively uniform internally yet different from adjacent periods. Each tribal history was as long as could be constructed by using modern archaeological, linguistic, ethnohistorical, and field work evidence. This made it possible to isolate about 22 different periods in the six cases. These periods provided the basic units used for comparative purposes.

Second, in defining periods as well as in comparing them, the seminar sought to conceptualize those aspects of contact situations which are most significant for culture change, such as the difference between directed and nondirected situations. These features were used as a basis for formulating tentative types of contact communities. Finally, there was the even more difficult problem of characterizing processes or types of cultural change. This was approached by examining the different ways elements from different traditions combine and become culturally integrated. Processes such as incorporation, assimilation, fusion, and isolation are distinguished and analyzed, and limited hypotheses about their relations to types of contact situations are suggested.

This significant book thus pioneers one of the first comparative methods which seems able to successfully assimilate the complexities of acculturation data.

ROBERT S. MERRILL
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Uganda's Labor Patterns

Migrants and Proletarians. Urban labour in the economic development of Uganda. Walter Elkan. Oxford University Press, New York, 1960. x + 149 pp. Illus. \$3.40.

This volume adds substantially to our knowledge of Uganda, a part of East Africa for which the documentation is already quite good. Elkan's title is derived from the fact that in the urban areas of Uganda (the study is based more particularly on the larger towns of eastern Uganda—Kampala and Jinja especially) two distinct groups of employed people are found. The migrants, drawn from all parts of Uganda and from the outside as well, are only temporary residents in the towns. The proletarians are mainly those indigenous to the immediate areas

in which the towns have grown up. They are local people, heavily dependent upon the wages they earn, for whom subsistence agriculture is either unattractive or impossible. For migrant workers, of course, wages are a supplement to farm income.

This is a pattern which, with substantial variations, is familiar elsewhere in Africa and in parts of Asia as well. The special merit of Elkan's work is that he questioned the usual assumption that short-term migration is essentially a transitional factor. He concludes that there are some decidedly positive advantages to migrant labor and that it is likely to persist.

Elkan provides the reader with a brief statement of his problem and with a summary description of Uganda, before proceeding to his more detailed discussion. The main part of the book provides a carefully drawn account of labor patterns: where and under what circumstances people are employed, variations in skill and in wages, the place of trade unions, the problem of labor efficiency, and the ways in which management enters into the picture. Since the employees are primarily Africans and the employers are ordinarily either Europeans or Asians, discussion of the former is most detailed.

Elkan draws useful comparisons with past and present English experience wherever possible and thereby highlights his own account. His writing is lucid, points which might be obscure to the nonspecialist are usually explained, and the implications for areas other than economics are often indicated. This book should appeal to—and be read by—a very wide audience.

ALFRED HARRIS

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Schools in Focus

The Schools. Martin Mayer. Harper, New York, 1961. xviii + 446 pp. \$4.95.

"Why another book about the nation's schools?" Some hard-pressed educators may add, "And why one by an outsider?" There has been so much analysis, comparison, attack, and defense in other recent volumes, what more is there to say?

One need read only a few pages of

The Schools to find that these are the wrong questions. Let the author himself explain:

"It was because I had some sense of the unreality of the 'great controversy' in education that I wanted to write this book. . . . The critic shouts that the schools are lazy and the educators fools; the educator shouts back that the critics are reactionaries and their criticisms are ignorant. . . .

"What follows is the result of some thirty months of observing, interviewing, reading and (briefly) teaching. The work has taken me as far east as Helsinki and as far west as San Francisco. I have visited about 150 schools and spoken privately with more than fifteen hundred people involved in one way or another with the education of the young. I have read forty or fifty linear feet of books and an uncounted weight of periodical literature. I would not say that my preparation was sufficient, but six thousand pages of notes are all the file will hold; and there comes a time when a man who writes for a living must sit down and write his book.

"The effort throughout has been to get at the realities of education, to cut below the controversy to the problems as they present themselves inside schools both in the United States and in some countries of Western Europe. Shortly after undertaking the job, I spent three months attempting to find and remove my own biases on this subject, so that I might enter the schools with as neutral a view as I could achieve."

From this neutral, reporter's background Mayer has produced a book that rates an A for reading by teachers, school administrators, critics, or parents. With sympathetic understanding instead of passion, in clear vivid English instead of jargon, with frequent specific examples, he describes the nation's schools, what they are like, how varied they are, how they became the way they are, the standards of quality expected, the preparation of teachers, the examination system and the uses of intelligence and other standardized tests, the teaching of different levels from kindergarten through senior high school, and the teaching of the native tongue, mathematics, science, the social studies, and foreign languages.

In all of this, Mayer's objective is to describe and analyze rather than to praise or blame or preach. He analyzes with skill, gives enough of the history of current problems to provide background, and intersperses illuminating

vignettes from classroom observation in European and American schools. He contrasts some of the exciting and imaginative things being done in some schools with the dull and dreary work going on in others.

Two ideas emerge with special clarity. Neither is new, but both have been buried in much of the shriller writing. One is that the schools are extremely varied; they cover the whole range from indefensibly stupid to exceedingly good. The other is that slowly, gradually, but cumulatively, the schools improve. But if this conclusion is hopeful, it is also sober. Improvement requires better teachers, better preparation of teachers, better teaching materials, better understanding and support on the part of school board and society. The excellence of many schools indicates that we know how to improve others, but, in Mayer's words, "the damnable difficulty is that all those connected with schools insist on behaving like people."

DAEL WOLFLE

American Association for
the Advancement of Science

Miscellaneous Publications

(Inquiries concerning these publications should be addressed not to Science, but to the publisher or agency sponsoring the publication.)

Atomic Energy Levels in Crystals. National Bureau of Standards Monograph 19. John L. Prather. 88 pp. \$0.60 (order from Supt. of Documents, GPO, Washington 25).

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Reports

Influence of Soil Oxygen Concentrations on the Development of *Meloidogyne javanica*

Abstract. Roots of tomato plants infected with *Meloidogyne javanica* were grown in soil subjected to oxygen concentrations of 21 percent, 5.5 percent, 3.5 percent, 2.0 percent, 0.6 percent, and 0 percent for 28 and 35 days. The lowest oxygen tension which still allowed development of the host and the nematode was 3.5 percent. Below this level the plant root growth, size of developing females, and production of nematode eggs were reduced. Nematode activity as measured by the number of nematode galls on the roots of treated plants was sharply reduced at the 5.5-percent level of oxygen.

Little information is available on the activities of plant-parasitic nematodes under various soil-oxygen tensions. Failure of *Heterodera schachtii* eggs to hatch was thought by Wallace (1) to be due to an oxygen shortage in the soil, caused by blockage of the soil pore system by excess water. Stolzy, Van Gundy, and Letey (2) have shown that numbers of *Meloidogyne incognita*, *Trichodorus christiei*, *Tylenchulus semipenetrans*, and *Xiphinema americanum* surviving in different soils were reduced after exposure to 0 percent oxygen for 10 days. Hatching of *T. semipenetrans* eggs was reduced after 3 days at 0 percent oxygen. The respiration of some free-living and plant-parasitic nematodes has been determined in various artificial atmospheres (3). These studies have shown that respiration under these conditions was independent of oxygen tension, and dependent upon tempera-

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should not repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to one 2-column figure (that is, a figure whose width equals two columns of text) or to one 2-column table or to two 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to contributors" [Science 125, 16 (1957)].

ture and presence of CO₂. It was also observed that the respiration decreased with length of time the nematodes were removed from soil. A method and apparatus for varying the oxygen concentration above the soil surface has been described by Stolzy *et al.* (4).

Tomato seedlings were infected by planting in small containers of silica sand infested with 500 larvae of *Meloidogyne javanica*. After 48 hours the seedlings were removed from the infested sand, transplanted in plastic cylinders containing 1 liter of steamed sandy soil, and grown for 3 weeks. The soil bulk density was uniformly adjusted in each cylinder to 1.65. Nematode development on duplicate plants had reached five and ten egg masses per plant, each containing approximately 100 eggs. The containers were sealed with the tops of the plants extended through holes in tightly fitted lids. The soil and roots were then subjected to the following oxygen concentrations derived by mixing air with nitrogen: 21, 5.5, 3.5, 2.0, 0.6, and 0 percent. One experiment ran for 28 days and another for 35 days after the treatments were applied. There were a total of six replicates per treatment. Moisture control was based on the weight-pot technique for keeping the soil between 18 and 30 percent (volume basis). This represents approximately 50 percent and 80 percent, respectively, of the total soil porosity filled with water. At the termination of the experiment the roots were washed from the soil and the nematode galls were counted. Half of the galls on each plant were stained with acid fuchsin, and ten developing females were removed and measured for size as described by Bird (5), except that the area was determined with a planimeter. The other half of the galls were placed in a vial for 11 days, after which the total number of larvae were counted.

The effects of the different oxygen tensions on the growth of the tomato plants in two experiments are similar to those already described by Letey *et al.*

(6). Plant growth (Fig. 1) and water use were reduced with a decrease in oxygen concentration. Plants grown at 0 percent oxygen were barely alive, while those at 21 percent oxygen were vigorous.

The number of nematode galls produced by secondary infection was significantly reduced between 21 and 5.5 percent oxygen (Fig. 1). However, the root growth was not significantly reduced above 3.5 percent oxygen. Possibly the hatching of eggs and the activity and ability of the nematode to infect plant roots in soil were affected by greater oxygen concentrations than required to affect root growth. Oxygen concentrations which significantly reduced the growth of the plant roots also caused a significant reduction in the size of the developing female nematodes.

The area, determined by outline drawings of female nematodes dissected from galls of plants grown at 0.6 percent and 0 percent oxygen tensions, was significantly smaller (127,000 μ^2) than those from plants grown at 21 percent (213,000 μ^2) (Fig. 1). Nematodes measured at the beginning of the oxygen treatments averaged 162,000 μ^2 . These measurements indicate that nearly all the nematodes measured at the 0.6 percent and 0 percent levels were of the second generation.

Although there was no significant difference in the size of the nematodes in plants grown at oxygen tensions of 21, 5.5, and 3.5 percent, the average number of larvae hatched per egg mass

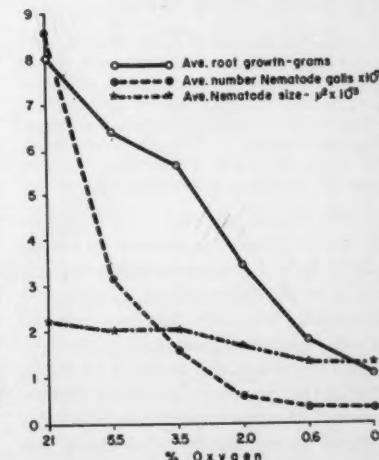


Fig. 1. Average tomato root growth, number of root-knot galls, and size of female nematodes grown at various oxygen concentrations for 28 and 35 days.

after 11 days incubation was 235, 130, and 63, respectively. Only a few eggs were found within the host plants grown at 2 percent oxygen.

The large reduction in number of galls at the lower oxygen levels appears to be due to the reduced hatching rate and development of the eggs of the original females and the reduction in infectivity of the larvae in the soil. The possibility that soil aeration affects nematodes has long been recognized; however, it has been difficult to establish direct evidence that inadequate oxygen in the soil limits the activity of nematodes except under rather extreme conditions. These results indicate that the activity of some nematodes in the soil phase is more dependent upon the availability of oxygen than previously thought.

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3 April 1961

Simultaneous Appearance of Free Arginine and Deoxyribosidic Compounds during Mitosis

Abstract. Sequential measurements were made of the free amino-acid pool in the premitotic region of microspores of *Lilium longiflorum*. The basic amino acid arginine appears in the pool and then disappears. Arginine appears at the same developmental stage at which it has been reported that the free deoxynucleosides appear.

The mitotic division in the microspores of *Lilium longiflorum* presents a particularly favorable subject for the study of the chemistry of mitosis. It is especially favorable for two reasons. First, a morphological index of development (1) orders the sequence of events during the interphase preceding mitosis; second, the microspores undergo this mitosis in synchrony (1). Using this material, Foster and Stern (2) and Taylor (3) have been able to show the precise time, during the interphase preceding mitosis, at which deoxyribo-

Table 1. Change in concentration of the free amino acids in the anther, with respect to the length of the flower bud in *Lilium longiflorum*. Mitosis of the microspores occurs at 60 mm.

Bud length (mm)	Change in concentration (μ mole/anther)		
	Lysine	Histidine	Arginine
50	4.67	0.32	
52.5	1.25	5.10	2.25
54	2.40	0.31	
55	1.62	2.10	
60	3.40	7.30	

nucleic acid (DNA) is synthesized. Nasatir, Bryan, and Rodenberg (4) showed changes in the composition of the soluble proteins during interphase and mitosis. The large quantitative change in the free deoxyribosidic compounds (2) and the correlation of this peak value with the doubling of one of the soluble proteins (4) led us to an examination of the associated changes in the free amino acids.

Flower buds of *Lilium longiflorum* Thunb. cv. "Croft" of varying lengths were removed, and the anthers were excised. The anthers were homogenized in 70-percent ethanol and centrifuged. The supernatant fraction was evaporated and the residue was dissolved in 2.5 ml of pH 2.2 buffer. The amino acids were separated by column chromatography by the methods of Moore and Stein (5). The quantity of each amino acid was determined by the ninhydrin reaction described by Moore and Stein (6).

Changes in many amino acids were found, but the most striking were the changes of the basic amino acids, lysine, histidine, and arginine. A measurable amount of arginine was present only at a bud length of 52.2 mm. This is the same bud length at which Foster and Stern (2) found free deoxyribosidic compounds and at which Nasatir, Bryan and Rodenberg (4) found the doubling of one of the soluble proteins. Histidine and lysine, in contrast to arginine, were present at all the bud lengths measured: 50, 52.5, 54, 55, and 60 mm. At the time of DNA synthesis, arginine appeared and histidine increased over tenfold, but lysine decreased by a factor of four. Later, at the time of mitosis, arginine was absent, but the concentrations of both lysine and histidine tripled. These results are summarized in Table 1.

Because of the small number of buds analyzed, it is difficult to say definitely how the pattern of the free amino-acid pool changes with development. For example, it is not possible to decide

whether the high level of arginine is present during all the premitotic fluctuations in free deoxyribosides or whether the concentration follows these fluctuations in detail. If the concentration fluctuates, there may be more than one peak. To demonstrate such a second peak would require the use of a much larger number of buds.

On one point, however, the data are quite unambiguous: Two amino acids, distinctively present in chromosomal protein, undergo marked concentration changes close to the time of DNA doubling. This result leads us to speculate that the pertinent protein synthetic mechanisms undergo a corresponding change related to the requirements of chromosome duplication.

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5 June 1961

Virus-Tumor Synergism

Abstract. More than 30 mouse tumors are associated with virus-like agents that may readily be detected by enzymic techniques. Radiation and chemically induced tumors do not ordinarily give evidence of such activity. The present report is of experiments in which a synergistic effect has been observed to occur when animals were inoculated with both the filtrable agent and a virus-free tumor. Synergism was shown by accelerated growth of the tumor and by elevation of a glycolytic enzyme (lactic dehydrogenase) in the host plasma.

Most transplantable mouse tumors and their hosts have been shown recently to have a virus-like agent, or agents, associated with them (1, 2). While these tumor-host-virus associations now include over 30 varieties of tumor types as revealed by enzymic techniques, the relationship of these agents to the neoplastic process is still uncertain (2).

Radiation-elicited and chemically induced primary tumors, in contrast, do not ordinarily exhibit evidence of such agents by these methods, and the significance of the exceptions which we have observed are obscure and require further critical studies. However, synergistic effects have been observed when a lactic dehydrogenase elevating agent obtained from a transplantable tumor, such as the Ehrlich carcinoma, was added to an apparently "virus-free" tumor-bearing host. Such newly derived primary tumors or their subsequent "virus-free" passage transplants, or other "virus-free" neoplastic equivalents, appear to be required for detection of such viral effects on tumor growth and metabolic response.

As shown in Fig. 1, the growth rate of the virus-infected 20-methylcholanthrene induced tumor was substantially increased in subsequent transplant passages compared with its nonviral counterpart. Appropriate histological differences consistent with more rapid cell division were also observed, and a glycolytic enzyme, lactic dehydrogenase, was substantially elevated in the host blood plasma when the Ehrlich-associated virus was inoculated into "virus-free" tumor-bearing mice (Fig. 2). This synergistically induced blood enzyme elevation was in considerable excess over that produced additively by the tumor or the virus alone.

Although the metabolic and other alterations appear to be permanent in the sense of being transmitted by repeated tumor transplant, it is unknown whether transduction or transformation phenomena are involved, since the intact agent continues its association with the tumor during such passages. In the present experiments, the enhanced tumor growth rate and the exorbitant increase in lactic dehydrogenase was a consequence of the controlled addition of a virus, while in the previous studies (1) covering a spectrum of 30 long-established transplanted mouse tumors the viral agents were already present through an unknown process.

The present findings provide a synthetic reproduction of the previously reported five separate phases of the serial plasma lactic dehydrogenase curve found associated with the malignant process in a wide assortment of standard transplanted mouse tumors (3). The numbers within circles in Figs. 1 and 2 indicate the various induced five-phase counterparts in these studies: Phase 1 constitutes the normal lactic dehydrogenase value during the post-

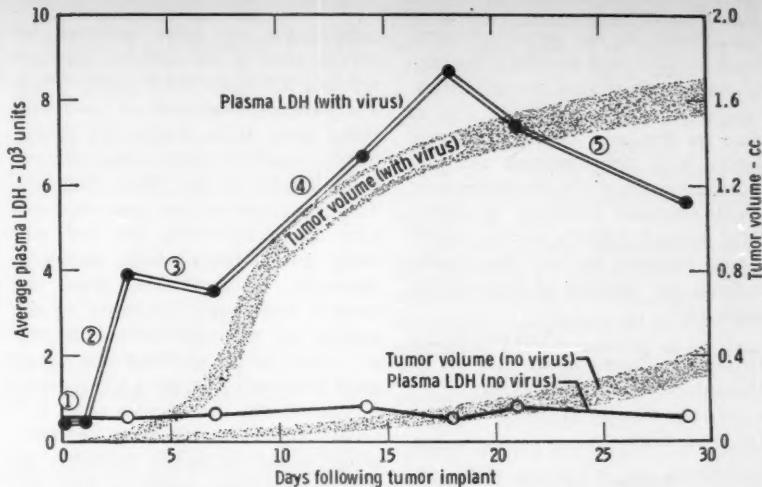


Fig. 1. Comparison of host plasma lactic dehydrogenase activity (LDH) and tumor growth rate in a 20-methylcholanthrene induced transplanted tumor with and without added virus obtained from a mouse bearing the Ehrlich carcinoma. Tumor in third transplant generation, agent added during primary induction.

injection latent period, phase 2 is the abrupt increase in this enzyme occurring between 24 and 72 hours after injection of virus in a normal host, phase 3 is a five- to tenfold elevated lactic dehydrogenase plateau and represents host-virus equilibrium when only the virus is present, phase 4 indicates the synergistic metabolic reaction between virus and established or growing tumor, and phase 5 reflects a quantitative reversal of this synergistic effect during the terminal stage of the host and the deteriorating stage of the tumor. This experimentally controlled virus-tumor

recombination may therefore constitute a general explanation of the previously reported correlations observed between mouse tumor growth and alterations in the host plasma lactic dehydrogenase. These data also demonstrate the ability of certain virus-like agents to affect the biological behavior and the biological and the metabolic characteristics of tumors irrespective of the question of their viral etiology.

Such virus-tumor synergism suggests new possibilities and approaches for examining the mechanism of lactic dehydrogenase and other enzyme eleva-

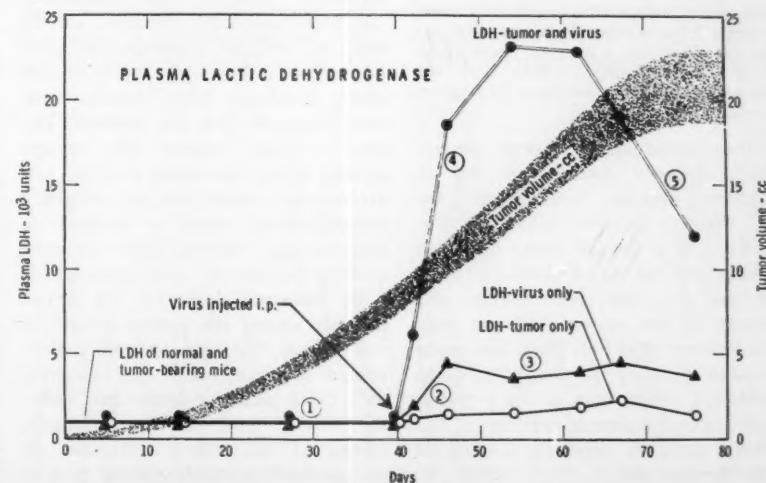


Fig. 2. Synergistic production of high plasma lactic dehydrogenase activity (LDH) in a "virus-free" tumor-bearing mouse after intraperitoneal injection of a virus-like agent associated with the Ehrlich carcinoma. The "virus-free" tumor is a radiation-induced transplantable pituitary neoplasm.

tions found in the serum of some cancer patients, and possibly other clinical states such as viral hepatitis. However, it should be mentioned that in mice we regularly observe moderate plasma lactic dehydrogenase elevation in certain stages of the development of radiation-induced leukemia and chemically induced primary tumors where, in most instances, we have been unable to detect the presence of transmissible agents (4).

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22 May 1961

Transreplication and Crossing Over in *Sordaria fimicola*

Abstract. A study of the segregation of markers closely linked to the gray ascospore color locus in *Sordaria fimicola* reveals that there is a high incidence of crossing over very near the locus when it transreplicates, which is much more pronounced in 5:3 than in 6:2 ascospores. Also, a single 7:1 and several aberrant 4:4 ascospores are described. At a different spore color locus, transreplication yields only 6:2 ratios, while other spore color loci fail to transreplicate altogether.

Transreplication or "gene conversion" is easily detected in ascospores of *Sordaria fimicola* heterozygous for loci affecting ascospore color (1). Recently (2), a linkage group has been worked out for the chromosome bearing the gray (g) color marker, and several of the mutant loci are near the g locus (Fig. 1). This has made possible a study of the relationship between crossing over in the g region and transreplication at that locus. The present report is based on a study of 146 aberrant ascospores.

An abnormal ratio for spore color appears among ascospores heterozygous for the g locus with a frequency of 1 in 800 to 1000 ascospores, regardless of the

combination of other markers involved. Most of the aberrant ascospores have 6:2 and 5:3 spore color ratios. These are generally believed to have resulted from DNA miscopying during pairing prior to meiotic separation of chromosomes in the ascospore (Fig. 2). The frequency of the two types is very nearly the same, the 5:3 ascospores being only slightly more abundant. However, transreplication does not proceed with equal frequency to the mutant and wild type alleles, the $g \rightarrow g+$ event occurring about five times more frequently than the $g+ \rightarrow g$ event.

An attempt to explain the 5:3 octads requires utilization of an eight-strand model of paired homologous chromosomes at meiotic prophase, such as that recently proposed by Taylor (3). A single half-chromatid temporarily switching from its own strand to copy for a short distance off the homologous strand can readily account for the 5:3 ratio (Fig. 2A) without the complication of chromosome breakage, rejoining, and obligately increased crossing over required by Taylor's explanation of the 6:2 event.

An examination of the data on crossing over in the region of transreplication shows that none of the linked loci is altered in character by the transreplication event at g. Furthermore, the frequency of crossing over in the mi-cor interval of 4.4 units, which includes the g locus, is much higher in the aberrant ascospores—especially those with a 5:3 ratio—than in normal ones, and these crossovers in most cases involve the transreplicating strand. This relationship has been previously explained on the basis of intimate chromosomal pairing within a restricted region, under which condition both crossing over and transreplication are favored. The present studies support this concept as well as the hypothesis that the two are separate events and not obligately interdependent, since a number of aberrant ascospores fail to show crossing over in the area of transreplication.

By variously deploying the linked markers among the strains crossed, it was found that the transreplication-related crossing over was occurring very close to the g locus and within the mi-cor interval. For example, among 61 ascospores with a 5:3 ratio, 41 (67 percent) showed crossing over in the mi-cor interval (4.4 units), 26 showed a crossover between g and cor (3.4 units), and 29 between mi and g (1 unit), with 14 (23 percent) of

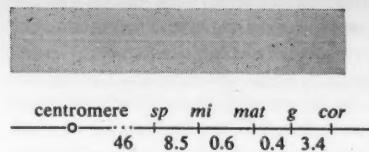


Fig. 1. Part of g linkage group showing morphological markers. Abbreviations: sp, spotty; mi, milky; mat, matted—all referring to mycelial characters; g, gray ascospore character; cor, corona, referring to the ring that appears near the center of the colony.

these having crossovers in both intervals simultaneously where only 0.14 percent of the ascospores would be expected to show double crossovers. When the marker mat just 0.4 crossover unit to the left of g was used, it was discovered that transreplication-related crossing over on that side was limited to the shorter interval.

In 6:2 ascospores the frequency of crossing over in the region of transreplication, though somewhat higher than normal, is less than half that found in 5:3 ascospores. This significant difference is not predicted by previous explanations of aberrant tetrads.

Six 4:4 ascospores, each with a pair of

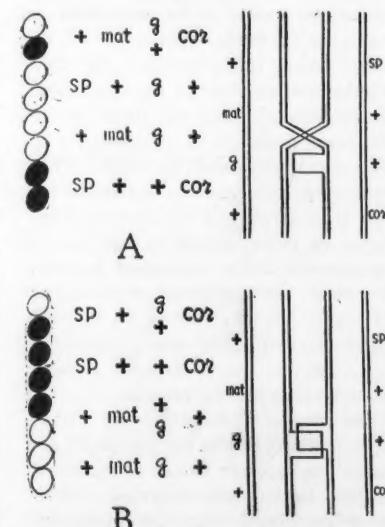


Fig. 2. Two types of aberrant ascospores with explanatory diagrams. (A), 5g:3g+ ascospore, whose progeny can be explained by a half-chromatid miscopying at the g locus and a crossover between mat and g. (B), Aberrant 4:4 ascospore that is most simply explained by reciprocal miscopying by two homologous half-chromatids.

spores heterogeneous for color in each end of the ascus, were obtained during the course of this study (Fig. 2B). Analysis of the progeny demonstrated that these had not resulted from drastic nuclear or spore rearrangement, since other loci segregated in normal pairwise manner. The data indicate that two half-chromatids of paired homologs switched over in opposite directions and miscopied alleles simultaneously. We are referring to this as "reciprocal double transreplication."

A single $7g+1g$ ascus was found. In this ascus other loci segregated normally. The unusual ratio is believed to have resulted from the combination of a 6:2 and a 5:3 event, but the two are difficult to explain as products of the same meiotic process. In view of this and the fact that 6:2 and 5:3 asci show a marked difference with regard to related crossing over, it may be that the two events have a different explanation, at least as to time of occurrence. If a 5:3 event occurred in the crozier within a diploid nucleus or between homologs of an intimately associated dikaryon which then divided once before producing the ascus nucleus, such an event would allow for a second DNA replication in the ascus and result in a 6:2 ratio. A 5:3 event in the ascus nucleus superimposed upon the foregoing occurrence could then give a 7:1 ratio. The data presently available are insufficient to determine whether this hypothesis is correct. If it should prove valid, then it would logically follow that transreplication in general is the result of miscopying by half-chromatids.

We have obtained several spore color mutants whose loci show no evidence of transreplication, possibly because they represent deletions or other chromosomal aberrations that are not subject to miscopying. On the other hand, one of our spore color mutants (*m*), when crossed with wild type, gives rise to 6:2 asci at the rate of about 1 in 1500, but no 5:3 asci have been found.

It is hoped that future studies may help explain these differences that have been observed among loci that are able to transreplicate and that they may further elucidate the underlying mechanism of the process (4).

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17 May 1961

Stimulation of Uterine Contraction by Extracts of the Cockroach, *Periplaneta*

Abstract. Blood and extracts of gut of the American cockroach, *Periplaneta americana*, powerfully stimulate contractions of the isolated rat uterus. The contractions are characterized by long latency and a prolonged relaxation phase having superimposed rhythmic contractions. Evidence indicates that the stimulating factor is not acetylcholine, 5-hydroxytryptamine, histamine, or substance P. The active factor is heat labile and nondialyzable.

Stimulation of the rat uterus by blood of the roach, *Periplaneta americana*, was encountered by Barton Browne *et al.* (1). To determine the origin and characteristics of the stimulating principle, extracts were prepared from brain, nerve cord, muscle, gut, and blood of roaches, and were tested on the isolated rat uterus preparation. The roach tissues were ground in a small mortar with Tyrode's solution, then filtered. Undiluted blood was obtained by centrifugation of the roaches (2).

In each experiment, one uterine horn of a rat was suspended in a 7-ml bath containing low-calcium Tyrode's solution (3), and its isotonic contractions were recorded by a conventional lever exerting a slight load on the uterus. The

muscle was stimulated at 4-min intervals by application of 0.5 to 2.0 μ g of carbamylcholine chloride (carbachol). Extracts were applied 4 min after the routine stimulation and were allowed to remain in the bath for 60 to 90 sec. Thereafter, washing was repeated at 60-sec intervals until the next application of carbachol.

Only gut extracts and blood showed the characteristic stimulating activity, with gut showing greater activity per unit weight and being used in most experiments. Uteri responded to extracts from as little as one-tenth of a single washed gut. Figure 1 shows typical responses. The latent period after introduction of the extract into the bath varies from 45 to 120 sec, depending upon dosage. Maximum contraction is followed by a long period of slow relaxation, with spontaneous contractions superimposed. Higher doses, equivalent to extracts of several washed guts, cause uteri to go into short contracture, followed by relaxation phases of up to 1 hr. The degree of uterine contraction obtainable far exceeds the maximum contraction obtainable by application of carbachol.

The active principle was differentiated from acetylcholine and histamine. Both rat colon and blood pressure were insensitive to extracts of ten roach guts. Atropine, sufficient to render uteri insensitive to carbachol, did not reduce the contractions elicited by gut extracts. Uteri responding strongly to extract equivalent to one gut were insensitive to 100 μ g of histamine, whereas the arterial pressure of the rat and cat showed a significant fall to only 1 to 2 μ g of histamine but did not respond to large amounts of the extract.

5-Hydroxytryptamine (5-HT) and

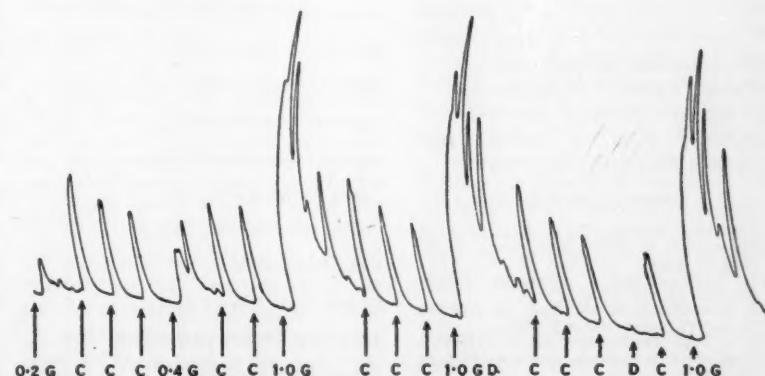


Fig. 1. The effects of dialyzed and nondialyzed extracts of roach gut upon isolated rat uterus. C, 1 μ g carbachol; 0.2 G and 1.0 G, extracts equivalent to 1/5, 2/5, and 1 roach gut, respectively; 1.0 GD, dialyzed extract equivalent to 1 gut; D, dialyzate equivalent to 5 guts.

substance P could also be ruled out as the stimuli concerned. Contractions induced by 0.1 μg of 5-HT exhibited short latencies and fast relaxations, and blocking of 5-HT responses of uteri by lysergic acid or dihydroergotamine (4) did not change the effects of the extracts. The insensitivity of the rat colon to the extract is also in agreement with this evidence (5). Substance P is known to occur in extracts of brain and intestine of some mammals and to induce slow contractions of a number of smooth muscle preparations (6), but it is insensitive to heat, is dialyzable, and stimulates rat colon. Active roach extracts were found to be nondialyzable through cellophane (Fig. 1), and to be destroyed by heating above 50°C for 10 min, although stable for at least 24 hr at pH 5, 7, or 8 at room temperature.

Smooth muscle stimulants with prolonged effects somewhat resembling those of the roach gut extract have been reported from several tissues. Dalgliesh *et al.* (5) extracted from the small intestine of the horse a material designated substance C, which caused long contractions of guinea pig ileum. Various workers (7) have reported the production, during anaphylaxis, of a smooth muscle stimulating substance which has been called "slow reacting substance." A nondialyzable, heat-labile extract from the abdomen of the garden tiger moth, *Arctia caja*, has been found to cause constriction of bronchial smooth muscle when given intravenously to guinea pigs (8). It is not yet known whether the activity reported here might be due to high concentrations of any of those substances in roach gut and blood (9).

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Succession in Desert Vegetation on Streets of a Nevada Ghost Town

Abstract. Vegetation was sampled on the old street system of Wahmonie, Nevada, and on a less disturbed area immediately adjacent. The vegetation on the denuded upland site showed a large increase in bunch-grass and an invasion by pioneer shrubs which ordinarily are chiefly confined to dry washes.

Muller (1) and Shreve (2) have concluded from studies in the Chihuahuan and Sonoran deserts that plant succession is essentially lacking in desert vegetation. According to Shreve (2), if a desert plant community is destroyed, the earliest stage in the return of the vegetation will be the appearance of young plants of the former dominants. "Not only do the same species appear at the outset, but their first individuals ultimately constitute the restored community." Nevertheless, on the Nevada

Test Site of the U.S. Atomic Energy Commission, in the northern Mohave Desert, there are a number of shrubs, sub-shrubs, and herbs which are unimportant on undisturbed upland sites in the desert, but which are ubiquitous weeds where the original vegetation has been destroyed by man. Many of the invading plants are natives of naturally disturbed desert habitats, chiefly dry washes; a few are weedy exotics, like *Salsola*. The Nevada Test Site is exceptionally well endowed with disturbed sites. Besides areas devastated by the detonation of nuclear devices, there are a variety of sites from which vegetation has been denuded by more prosaic, mechanical means. Unfortunately, most of the disturbed areas are of rather recent origin, dating from the past 10 years, or else are of indeterminate age. However, the discovery of horn silver in the Tertiary volcanics east of Jackass Flat in 1928 has fortuitously provided a quasi-experimental plot 33 years old. On the bajada south of Lookout Peak, Nye County, the townsite of ephemeral Wahmonie, Nevada, was platted as eight streets and five avenues (unpaved, ungraded), which are still in evidence, although buildings have long since disappeared. According to Murbarger (3), "automobile-borne miners began pouring out of Tonopah toward

Table 1. Comparison of desert vegetation on an abandoned street system 33 years old with that of an adjacent, less disturbed site. Density is given as plants per acre and frequency as percentage.

Streets of ghost town	Density	Frequency	Adjacent site	Density	Frequency
<i>Stipa speciosa</i> Trin. & Rupr.	1050	96	<i>Grayia spinosa</i> (Hook.) Moq.	1082	100
<i>Ephedra nevadensis</i> Wats.	378	86	<i>Lycium Andersonii</i> Gray	565	96
<i>Lycium Andersonii</i> Gray	351	74	<i>Larrea divaricata</i> Cav.	344	80
<i>Thamnosma montana</i> Torr. & Frém.	153	48	<i>Ephedra nevadensis</i> Wats.	264	70
<i>Hymenoclea Salsola</i> T. & G.	108	32	<i>Coleogyne ramosissima</i> Torr.	201	42
<i>Salazaria mexicana</i> Torr.	81	20	<i>Stipa speciosa</i> Trin. & Rupr.	138	44
<i>Grayia spinosa</i> (Hook.) Moq.	45	20	<i>Thamnosma montana</i> Torr. & Frém.	21	8
<i>Acamptopappus Shockleyi</i> Gray	32	8	<i>Acamptopappus Shockleyi</i> Gray	5	2
<i>Larrea divaricata</i> Cav.	18	6	<i>Eurotia lanata</i> (Pursh) Moq.	5	2
<i>Coleogyne ramosissima</i> Torr.	14	6	<i>Krameria parvifolia</i> Benth.	5	2
<i>Dalea Fremontii</i> Torr.	4	2	<i>Opuntia acanthocarpa</i> Engelm. & Bigel.	5	2
<i>Eurotia lanata</i> (Pursh) Moq.	4	2	<i>Yucca brevifolia</i> Engelm.	5	2
<i>Aplopappus Cooperi</i> (Gray) Hall	4	2			
<i>Krameria parvifolia</i> Benth.	4	2			
<i>Lepidium Fremontii</i> Wats.	4	2			
Total	2250		Total	2640	

the new bonanza, and in April, 1928, two months after the original strike, the town had a population of 1500, the Gilbert brothers had installed an electric light plant and numerous stores and cabins had been built. Almost before it was possible to say 'Wahmonie, Nevada,' the boom collapsed." The townsite is located at an elevation of 4300 feet on a gentle south-facing slope, on unsorted alluvium typical of fans in this area, but free of major drainageways. The soil has a gravelly, granular, sandy-loam texture with lag gravel on the surface, the whole derived from the lavas and tuffs of the adjacent mountains. The platting of the townsite apparently amounted to little more than clearing of the shrubs from the street system, but the "streets" are still conspicuous because of the peculiarity of the vegetation which has become established on them in the course of 33 years. The perennial vegetation was sampled on the streets and on the less disturbed portion of the bajada immediately adjacent, which was taken as the best available "control." A distance method similar to the quarter method of Curtis and Cottam (4) was used, but ten individuals were recorded per point. Fifty points were taken, giving a total sample of 500 individuals for each site. The results are presented in Table 1.

Clearly, there have been pronounced changes in the physiognomy and floristic composition of the vegetation in Wahmonie. In fact, an open stand of bunch-grass has partially replaced the desert shrub community, because of the large increase of *Stipa*. On the whole, invasion by shrubs has been slow, and the species composition has changed both quantitatively and qualitatively. Of the dominant shrubs on this portion of the bajada, only *Ephedra* and *Lycium* are present on the townsite in large numbers, and it happens that both of these are species which sprout vigorously from underground parts when the tops are destroyed. It is significant that *Thamnosma*, *Hymenoclea*, and *Salazaria*, which are characteristic shrubs of naturally disturbed desert habitats, such as drainageways and actively eroded bedrock areas, should appear in large numbers on a man-disturbed site. This establishes a pioneer or weedy character for certain plants of dry washes, which have been regarded as an *ad hoc* climax. Shreve (2) concluded: "Each habitat in each subdivision of a desert area has its own climax, which must not be given an elastic definition and must be inter-

preted as having a genetic relation to any other climax." Since the word climax, as applied to vegetation, conveys an idea of relative stability, it seems scarcely applicable in the case of desert washes, which are periodically scoured by floods carrying great quantities of coarse detritus. The average wash presents an extensive open surface favorable for invasion by pioneer plants possessing the requisite adaptations for efficient seed dispersal, celerity of growth, and early maturation. One might suppose that all plants of this habitat have a greater water requirement than plants of upland sites and for this reason are restricted to naturally irrigated areas of the desert. However, the present study shows that several shrubs of dry washes can become established in abundance on the more xeric upland portions of an alluvial fan when the competition of the dominant upland shrubs is largely removed. Obviously, these pioneer plants of the desert play a role similar to that of successional plants of more humid regions (5).

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Biosynthesis of Stigmasterol in Tomato Fruits

Abstract. The presence of stigmasterol in tomato fruits was demonstrated. Labeled mevalonic acid was incorporated into this sterol, while sodium acetate was not. The identity of the isolated product was established by rigorous purification to constant specific activity, melting point determinations, and infrared spectrum.

The biosynthesis of steroids in plants has only recently begun to receive attention, in contrast to the large volume of work on the biosynthesis of animal steroids (1). In 1958 Sander and Grisebach (2) reported the incorporation of labeled acetate into tomatin in tomato seedlings. Guseva *et al.* (3) have studied the biosynthesis of the sterol

Table 1. Purification and radioactivity of stigmasterol. Melting points were taken on a Kofler block and are corrected. Radioactivity was determined to ± 0.9 error.

Treatment	mp (°C)	Count/min mmole
<i>Stigmasterol acetate</i>		
A	142-144	3200
B	142-145	1870 \pm 90
C	143-145	1280 \pm 70
D	143-145	913 \pm 68
E	143-145	840 \pm 68
<i>Stigmasterol</i>		
F	168-170	733 \pm 49
G	168-170	766 \pm 58
H	169-171	770 \pm 54

idal alkaloids chaconine and solanine in potato sprouts, finding that mevalonic acid is no more effective as a precursor than acetate. Ramstad and Beal (4) injected labeled mevalonic acid into *Digitalis lanata* plants, from which they then extracted radioactive digitoxigenin. On feeding radioactive mevalonic acid or sodium acetate to *D. purpurea* plants, Gregory and Leete (5) found that, while both were incorporated into digitoxigenin, only the acetate was incorporated into the side chain. Recently Nicholas (6) showed that β -sitosterol is synthesized from labeled mevalonic acid by *Salvia sclarea*; the sterol from the flowers had higher radioactivity than that from other parts of the plant.

In our first work on the biosynthesis of plant steroids (7), we incubated *Dioscorea floribunda* tuber homogenates with labeled acetate and mevalonate; acetate was incorporated into diosgenin, but mevalonate was not. While it is probable that the steroid nucleus is formed in the same manner in plants and animals, more information is needed concerning the origin of the side chain in typical plant steroids. The present study was initiated by a separate investigation of the biosynthesis of carotenoids in *Lycopersicon esculentum* fruits from mevalonic acid (8), in which a highly radioactive sterol fraction was discovered.

Preliminary experiments were carried out on "Snowball" tomatoes (9). By injection of radioactive mevalonic acid by the technique described by Purcell *et al.* (8) and use of the separation procedure described by Heftmann *et al.* (10), we isolated pure stigmasterol, which had an infrared spectrum identical with that of an authentic sample. The yield of the sterol fraction was low, however, and the radioactivity was not high enough to allow dilution with carrier material. Accord-

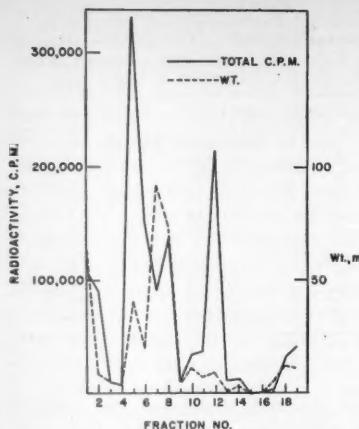


Fig. 1. Distribution of radioactivity in chromatographic fractions.

ingly, in all further work red tomatoes were used.

Thirty-one red tomatoes, weighing 4714 g, were injected with 2 μ C each of mevalonic acid-2-C¹⁴. After 24 hours, they were homogenized in a Waring blender with an equal weight of water, hydrolyzed by refluxing with concentrated hydrochloric acid (150 ml./lit.), and extracted with dichloromethane. The extracts were washed with 2N sodium hydroxide and evaporated. The residue was refluxed with phthalic anhydride in pyridine and the hemiphthalates of the alcoholic material were isolated by extraction with base. Hydrolysis of the hemiphthalates by refluxing with sodium methoxide solution gave 865 mg of a crude alcoholic fraction. This material was chromatographed on 30 g of alumina (11), grade III, taking the following 100-ml fractions: 1 to 2, 10-percent, 3 to 4, 25-percent, and 5 to 6, 50-percent benzene in petroleum ether; 7 to 9, benzene; 10 to 11, 10-percent, 12 to 13, 25-percent, and 14 to 15, 50-percent ether in benzene; 16, ether; 17, 1-percent, 18, 5-percent, and 19, 20-percent methanol in ether. A 0.1-mg aliquot of each fraction was plated on a copper planchet and the radioactivity was determined under a micromil window tube in an atmosphere of Q gas. The distribution of radioactivity in these fractions is shown in Fig. 1. Fractions 7 and 8 contained the sterol mixture, which proved much more difficult to purify than in the case of the white tomatoes. After preliminary chromatographing and crystallization, the material that melted above 140°C was combined and acetylated, and the acetates (42 mg) were diluted with 42 mg

of pure stigmasterol acetate. This material was purified by chromatography, crystallization from acetic acid, two more chromatographic separations, and recrystallization from methanol. At this point it weighed 8.2 mg, melted at 139° to 141°C, and had a specific activity of 17.8 count/min per milligram. After dilution with 9.6 mg of pure stigmasterol acetate, it was subjected to the following operations, as illustrated in Table 1. After each treatment, 2-mg aliquots were counted in duplicate to a 0.9-level of confidence. Treatment A was recrystallization from acetic acid. Treatments B, C, D, and E consisted of successive recrystallizations from methanol. The material from E was hydrolyzed with sodium methoxide in methanol, and the product (F) was recrystallized from methanol (G), and again from ethanol (H). These treatments gave stigmasterol having a constant specific activity of 770 count/min per millimole.

In another experiment, tomato fruits were injected with sodium acetate-2-C¹⁴ and worked up as before. In this case the sterol fraction was much less radioactive, and the pure stigmasterol finally obtained showed no significant radioactivity.

The presence of sterols in tomato fruits has not been reported heretofore and much remains to be learned about the biosynthesis of sterols in fruits. Unfortunately, the level of radioactivity of the stigmasterol obtained in these experiments was too low to permit degradation for establishing the pattern of labeling. In contrast to our previous findings on the biosynthesis of diosgenin in a tuber (7), it is now evident that mevalonic acid is a precursor of stigmasterol in tomato fruit. Acetate is apparently utilized so extensively for other biochemical reactions that only insignificant quantities are incorporated into stigmasterol under the conditions of our experiment.

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Chemical Analysis of Surfaces by Use of Large-Angle Scattering of Heavy Charged Particles

Abstract. The Rutherford scattering of charged particles from the heavier elements and nuclear scattering and (α, p) reactions from the light elements result in energy spectra that are characteristic of the nucleus being bombarded. A simple apparatus for analyzing surfaces based on these ideas can be made by using an alpha source such as Cm²⁴⁴, a solid state detector, and an electronic pulse height analyzer.

New methods of chemical analysis are always of interest, especially if they can be made more automatic than conventional techniques. This report calls attention to a nondestructive method, best used in vacuum, that is particularly applicable to the study of surfaces (the top 1 to 100 μ). The apparatus is very simple and the information is obtained in electronic form, which recommends the method especially for application at distances where the transmission of data is a problem.

It is proposed that scattering of a monochromatic, collimated beam of charged particles (for example, alpha particles from a thin radioactive source) be utilized for analysis of a solid. This method of analysis has been mentioned before (1), but the application of the newly developed solid state detectors makes it particularly attractive to both solid and particulate analysis.

For relatively low energy particles, such as alpha particles from the usual radioactive sources, and for those elements that are heavier than aluminum, the large-angle scattering of heavy charged particles is primarily Rutherford scattering. The energy of such scattered particles from a target thin enough so that the energy of the par-

ticles is not appreciably changed on traversing the target (less than 1μ in the case of the usual alpha particles on solids) is given by

$$T = T_0 \frac{[X \cos \theta + (1 - X^2 \sin^2 \theta)^{1/2}]}{(1 + X)^2} \quad (1)$$

where T_0 is the initial energy of the particle; $X = a/A$; a is the mass number of the beam particle (that is, 4 for alpha particles); A is the mass number of the scattering nucleus; and θ is the angle of scattering ($\theta = 180^\circ$ = backward scattering). The maximum separation between neighboring mass numbers is obtained when the scattering is backward. At this angle

$$T = \left(\frac{A - a}{A + a} \right)^2 T_0 \quad (2)$$

Thus the method has good resolving power for light elements, including the possibility of isotopic analysis.

The intensity of scattering at a given angle (see, for example, 2, p. 243) is determined primarily by the square of the nuclear charge (Z^2). It is a minimum backward, but the resolution advantage in that direction would appear to be the more important in many applications.

In the case of a thick target scatterer, even a monoisotopic target will give a

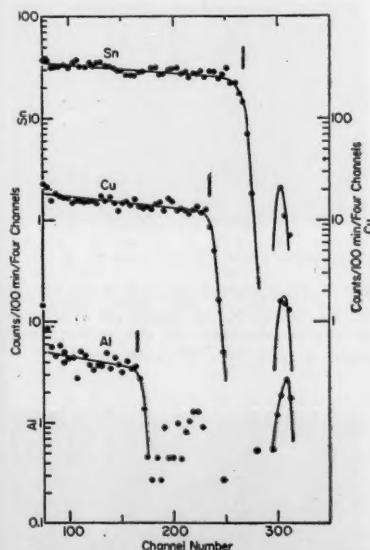


Fig. 1. Energy spectrum of alpha particles reflected from thick samples of ^{26}Al , ^{63}Cu , and ^{113}Sn . The ordinates are in counts per four channels per 100 min. The scales for Sn and Al are on the left; that for Cu is on the right. The abscissae are the channel numbers which are linearly related to the energy of the alpha particles. The vertical lines indicate the predicted high energy cutoffs for the elements in question.

continuous reflected spectrum starting close to zero energy. The high energy cutoff at a given angle, however, will be sharp and will occur at the same energy as with a thin target (Eq. 1). The spectrum calculated on the basis of well-known formulas (2, p. 101) for the stopping of charged particles has an essentially rectangular shape from about $0.1 T_0$ to the end point. The shape (except for the position of the end point) is practically independent of the target material. With a thick target, the intensity of Rutherford scattering will depend, in addition to the factors mentioned in the case of a thin target, primarily on the inverse of the atomic stopping powers (s) for charged particles of the target material.

These theoretical relations are valid for low-energy alpha particles or heavy elements. For light elements such as carbon and oxygen, and particularly with rather high-energy alpha particles such as those from Cm^{244} (5.80 Mev), nuclear effects enhance the scattering above that predicted from pure Rutherford scattering. This is accompanied by some distortion in the shape of the energy distribution. For other light elements, such as nitrogen and fluorine, protons from (α, p) reactions can also be expected.

The existence of a continuous spectrum from a single mass scatterer lowers the sensitivity of the method for analysis, since an observed spectrum from a surface containing several elements must be decomposed into components. The high-energy end points characteristic of each mass number are, however, unaffected by the chemical or physical state of the scatterer.

Some simple experiments have been performed to check these ideas, particularly as applied to thick targets. Two alpha sources in the range 10^6 to 10^{10} alphas per minute (3) were used together with surface depletion solid state detectors (4) and pulse height analyzers. The distance from source to scatterer and scatterer to detector was about 3 cm, and the average scattering angle was about 160° .

Figure 1 shows the energy spectrum of Pu^{239} alpha particles scattered from thick targets of ^{63}Cu , ^{113}Sn , and ^{26}Al . The energy of the original Pu^{239} is indicated by the slight amount of contamination producing a peak in approximately channel 305. The energy zero is at a negative channel No. 20.

Figure 1 shows that the shape of the energy distribution is similar to that expected. The observed high energy

cutoffs as well as the relative intensities from the different scatterers agree satisfactorily with theoretical expectations.

Figure 2 shows data obtained with another alpha source (Cm^{244}). Although more intense, the alpha particles of this source were not very monochromatic, as indicated by the source spectrum shown at the right of Fig. 2. This made the end points less sharp than in Fig. 1. Figure 2 shows spectra obtained with polyethylene (CH_2), quartz, calcium carbonate, and copper reflectors. Again, the positions of the end points correspond satisfactorily with those predicted. The shapes of the spectra are as expected from simple considerations, except in the case of carbon. For this element, the Cm^{244} alpha particles excite nuclear scattering levels of the $\text{C}^{12} + \text{He}^4$ system, and this results in a scattering some 40 times greater than Rutherford scattering. The scattering by O^{16} is also increased by similar effects by a factor of about 5 over that expected from Rutherford scattering.

The experimental work thus supports the theoretical expectations set forth in

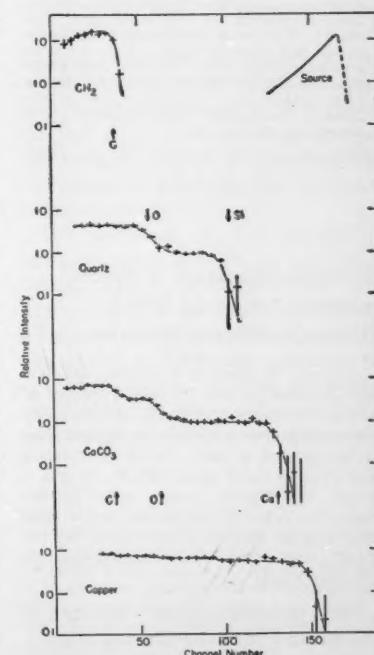


Fig. 2. Energy spectra of Cm^{244} alpha particles scattered at $\sim 160^\circ$ from various materials. The ordinates are in relative intensity units; the abscissae are channel numbers, linear in energy. The energy spectrum of the source is indicated at the top right. The theoretical end points for the constituent elements are shown by vertical arrows. The amplification was not exactly the same in the different experiments.

the first part of this paper. The shapes of the thick target spectra are approximately rectangular, with sharp high-energy end points. The positions of these end points is characteristic of the mass number of the scatterer. The positions of the end points are independent of the chemical state of the scatterer. Decomposition of scattering curves obtained by this technique can clearly give analytical information on the kinds and amounts of elements present in the scattering body (5).

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5. This paper is based primarily on a proposal made on 13 September 1960 to the National Aeronautics and Space Administration for the chemical analysis of the moon and other planets. I gratefully acknowledge the experimental help provided by Dr. H. L. Anderson and Dr. George Reed, and the very helpful discussions with Professors S. K. Allison, J. A. Simpson and N. Sugarman. Mr. A. Van Ginneken performed useful calculations connected with this problem.

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10-Methoxyharmalan, a Potent Serotonin Antagonist Which Affects Conditioned Behavior

Abstract. 10-Methoxyharmalan, an alkaloid obtained by the cyclodehydration of melatonin, itself a derivative of serotonin, is a more potent serotonin antagonist than harmaline and is only slightly less active than lysergic acid diethylamide. It has a similar, yet slightly greater, effect on behavior as that of harmaline and is the most potent serotonin derivative, so far tested, that affects the avoidance-escape behavioral reflex.

The psychotomimetic activity of lysergic acid diethylamide was originally postulated as being due to its ability to antagonize the action of serotonin (1). It has been further suggested that an endogenously produced serotonin antagonist might be responsible for some psychotic states (2). Melatonin has been isolated from pineal tissue (3), and its biogenesis from serotonin has been established (4). The removal of a molecule of water from melatonin

results in its conversion to 10-methoxy-harmalan (1-methyl-6-methoxy 3,4-dihydro-2-carboline), an analogue of harmaline (Fig. 1), which has been shown to be a serotonin antagonist (5). Harmine, a closely related compound, has been reported to be hallucinogenic (6).

Therefore, the effect of 10-methoxy-harmalan as a serotonin antagonist and on the behavior of trained rats was studied.

Serotonin antagonism was measured (i) on the isolated estrus rat uterus, (ii) on the isolated guinea pig ileum, and (iii) on the blood pressure of rats previously treated with ganglioplegic agents, atropine, and bilateral vagotomy.

The standard oxytocic response to 0.2 μ g of serotonin was completely blocked by the addition of 0.5 μ g of lysergic acid diethylamide, 2.0 μ g of 10-methoxyharmalan (Fig. 2), 50 μ g of harmaline (Fig. 3), or 50 μ g of harmine to a 10-ml muscle bath 5 min before addition of the serotonin. At higher dose levels, the harmala alkaloids frequently caused contractions, and it was noted that if a contraction was elicited the subsequent serotonin antagonism was decreased.

Similar results were obtained with the isolated guinea pig ileum. The dose response curve of the ileum to serotonin showed a plateau at 10 μ g of serotonin, the dose level at which the muscle strip contracted maximally. In the presence of 5 μ g of 10-methoxyharmalan, the dose response curve to serotonin was depressed, and the maximum contraction was less. Since the two curves were parallel, competitive antagonism could be postulated, and thus the action of serotonin and 10-methoxyharmalan on the same receptor site could be considered possible.

Preliminary studies of the effect of 10-methoxyharmalan on the blood pressure of ganglion-blocked rats indicate that it is a more potent vasodepressor than harmaline and that it antagonizes to some extent the pressor effect of serotonin.

The effect on behavior was assayed by using rats conditioned to an avoidance-escape schedule in a conventional shuttlebox. The number of mistakes was plotted against the intraperitoneal dose level of the compound used, ten animals being subjected to ten trials in each assay.

Melatonin caused no behavioral disturbance at dose levels of 0.2 mmole/kg. 10-Methoxyharmalan caused condi-

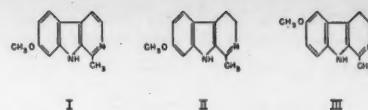


Fig. 1. Chemical structure of harmine (I), harmaline (II), and 10-methoxyharmaline (III).

tioned rats to make mistakes at doses as low as 0.008 mmole/kg with a linear dose response relationship up to 0.25 mmole/kg, at which level animals made ten mistakes out of ten trials. Harmaline exhibited a linear dose-response relationship parallel to, but slightly less active than, that of 10-methoxyharmalan, the dose level at which ten mistakes out of ten trials occurred being 0.28 mmole/kg. 10-Methoxyharmalan was thus approximately twice as potent as 5-methoxy-*N,N*-dimethyltryptamine and six times as active as bufotenine, both of which were previously tested in a similar fashion (7).

Rats given 10-methoxyharmalan at doses greater than 2 mg/kg exhibited tremor which lasted for approximately 1 hour, but were well able to walk at dose levels as high as 10 mg/kg.

Although lysergic acid diethylamide

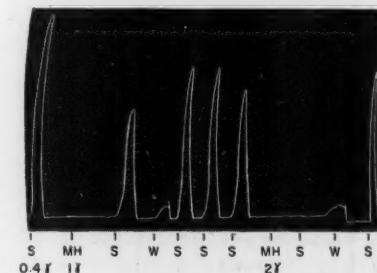


Fig. 2. 10-Methoxyharmalan (MH), 2 μ g, caused complete inhibition of oxytocic activity of serotonin (S), 0.4 μ g. Oxytocic response returned after washing (W).

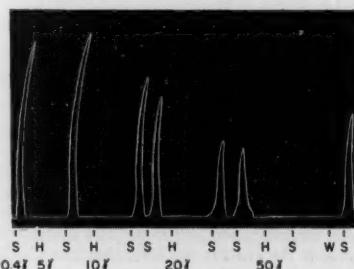


Fig. 3. Harmaline (H), 50 μ g, caused complete inhibition of oxytocic activity of serotonin (S), 0.4 μ g. Oxytocic response returned after washing (W).

is a potent serotonin antagonist and psychotomimetic compound, its psychotomimetic action may be related to, but is not necessarily dependent upon, its serotonin antagonism. Bromo-lysergic acid diethylamide, for example, is a potent serotonin antagonist, yet has little psychotomimetic action. It is, however, significant that the compound, 10-methoxyharmalan, is a potent antagonist of the myotropic action of serotonin and probably is a competitive antagonist. It is also the most potent derivative of serotonin, so far tested, that causes conditioned animals to make mistakes in an avoidance-escape schedule.

Unequivocal evidence for the production of 10-methoxyharmalan in the body has not been obtained, but it must be noted firstly that it can be derived from serotonin in three steps, the first two of which have been shown to occur in vivo, namely N-acetylation (8), O-methylation (4), and cyclodehydration. Secondly, a minor metabolite of melatonin previously noted (9) does not give the characteristic color reaction for indoles, and thus could be a cyclic derivative. Lastly, the highest concentration of serotonin has been found in the pineal glands of psychotic patients (10). These factors, in addition to the evidence presented here, tend to support the hypothesis that some psychotic states could be due to an endogenously produced harmala alkaloid (11, 12).

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12. We thank Mrs. Gertrude Britton for generous financial support and Mrs. Cornelia Brand for technical assistance.

11 May 1961

8 SEPTEMBER 1961

Eyestalk Movements Induced by Polarized Light in the Ghost Crab, *Ocypode quadrata*

Abstract. Differential visual sensitivity to vertical and horizontal linear polarization is shown in the light-induced eyestalk deviations of *Ocypode quadrata*. Responses with the *e*-vector vertical averaged about 6° greater than those with *e*-vector horizontal. This difference approximates the relative eyestalk deviation induced by unpolarized light intensities having a ratio of 3:1.

Although many arthropods have been shown to respond to linearly polarized light (1, 2), relatively little is known about the ability of higher crustaceans to see such polarization (3-5). Consequently, experiments were initiated on various decapods to help remedy this situation. The present report (6) describes differential responses to polarized light measured in terms of eyestalk movements in the ghost crab, *Ocypode quadrata* (Fabricius). Related experiments on learning and monotactic orientation in lobsters and crabs are reported elsewhere (7, 8). Except for studies on orientation of the whole animal, previous work on responses to polarization has been limited to observations on eye position in *Daphnia* (2, 9) and on electrical responses of the eye in insects (10-12) and horseshoe crabs (13).

The position of the eyestalks of decapod crustaceans is quantitatively dependent on the intensity and the incident angle of the prevailing illumination (14). Maximum deviation is elicited by unilateral horizontal light parallel to the animal's transverse axis. Furthermore, the extent of this eyestalk light response depends on the degree of statocyst excitation (15). With one statocyst removed, maximum eyestalk deviation evoked by light occurs when the gravity-induced shearing force in the remaining statocyst is minimized.

In many decapods this is achieved when the animal is tilted around its longitudinal axis about 30° towards the side without a statocyst. Consequently, in the present experiments the left statocyst was removed 2 to 5 days before use, and the crabs were fixed in a clamp holding them in this position for maximum eyestalk response (Fig. 1).

The light source was an automobile headlight (16) directed first through a small aperture and then through a rotatable polarizing filter (Polaroid HN38). This provided a narrow beam (1 to 2

mm in diameter) of white light nearly 100 percent linearly polarized. The tests reported here were limited to vertical and horizontal positions of the *e*-vector (plane of polarization). Full intensity at the eye was 800 lux without the polarizing filter and 620 lux with the polarizing filter; a lower intensity of 280 lux was obtained by using neutral filters made of uniformly exposed photographic film. Special care was taken in using this setup to eliminate reflection-refraction artifacts which might provide intensity cues for the plane of polarization.

The stimulating light beam was directed laterally at the crab's left eye (about 4 mm in diameter), which was fixed in position with paraffin to maintain a constant angle of light incidence. The response measured was the position (angle, α , between its long axis and the horizontal) of the right eyestalk, which was freely movable and not illuminated by the test light (Fig. 1).

Four individual mature specimens 25 to 30 mm in carapace width, were tested in this setup. Two types of comparisons were made. First, eyestalk responses were compared for vertical and horizontal positions of the stimulus *e*-vector with the intensity at 620 lux. Second, the intensity ratio of unpolarized light necessary to produce the same difference in eyestalk deviation as the two planes of polarized light produced was determined.

The results of the first experiment are summarized in Table 1. The mean eyestalk angles (averaged from 169 measurements under each condition) are shown for individuals exposed to vertical and to horizontal *e*-vectors. In all four animals eyestalk deviations were greater by statistically significant amounts (*P* values for the no-difference

Table 1. Influence of vertically (*v*) and horizontally (*h*) polarized light stimuli on the angle (α) between eyestalk axis and a horizontal plane. Means of the readings for each animal are given as well as their standard deviations (*s*) and differences ($\alpha_v - \alpha_h$).

	α°				
	<i>v</i>	<i>s</i>	<i>h</i>	<i>s</i>	$\alpha_v - \alpha_h$
<i>Animal 1 (71 observations)</i>					
	71.8	1.0	66.4	0.98	5.4
<i>Animal 2 (57 observations)</i>					
	89.3	0.6	80.3	1.7	9.0
<i>Animal 3 (16 observations)</i>					
	91.3	1.1	85.6	1.1	5.7
<i>Animal 4 (25 observations)</i>					
	90.2	0.7	85.3	1.0	4.9
<i>Mean</i>					
	85.7	—	79.4	—	6.3

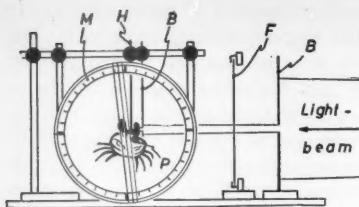


Fig. 1. Method of measuring the angle (α) between the right eyestalk axis and a horizontal plane when a small essentially parallel light beam stimulates the left eye laterally. B , diaphragm; F , linear polarizer; H , clamp; M , protractor; P , paraffin fixing the left eye.

hypothesis varied from .001 to .0001) with vertical polarization than with horizontal polarization of the same photometric intensity. On the average vertical polarization was more effective by 6°.

The intensity ratio ($I_1 : I_2$) of unpolarized light required to match approximately the differences in eyestalk deviations ($\alpha_r - \alpha_b$) evoked by the two polarization planes was found in a few experiments on one individual to be 3:1. For these results I_1 was about 800 lux, $\alpha_r - \alpha_b$ was 5.6° and $\alpha_{I_1} - \alpha_{I_2}$ was 5.2°. Variance of the readings was such that the differences in eyestalk deviations were significant in the two cases.

Because of the care taken to eliminate intensity artifacts, the observed difference in the effectiveness of the two planes of polarized light may be taken as evidence that the eye itself was acting as a polarized light analyzer. Earlier attempts to demonstrate polarized light sensitivity in decapod crustaceans have provided one somewhat doubtful report (17) or negative results (5); but since the present work was completed, extensive positive data have been obtained on orientation responses to polarized light by many kinds of decapods (8, 18). Also, previous efforts to demonstrate differential phototactic effects of vertically and horizontally polarized light failed with *Tenebrio* larvae, the beetle *Tetraopes*, and the terrestrial isopod *Cylindicus* (19).

At present neither the mechanism nor the biological significance of the polarized light responses reported here is known. Either a peripheral or central origin is possible for the observed lack of radial symmetry in sensitivity to plane of polarization (5, 20). In the insect *Notonecta*, a bilateral symmetry of the ommatidium is apparently involved since the amplitude of the electroretinogram in this plane of symmetry is about 22 percent greater than with the e -

vector 90° away (11). The only hypothesis for the mechanism of polarized light sensitivity consistent with the known facts requires individual retinular cells to be differentially sensitive to e -vector positions (3, 5, 10-12, 21). In dipteran insects, intracellular electrodes have demonstrated the required type of photoreceptor element (presumably a single retinular cell) with response maxima and minima to plane polarized light 90° apart (12, 21). The intensity differences necessary to match these maxima and minima are rather similar to those found in the present data (22).

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9 March 1961

General Method of Plotting Kinetic Data for Reactions of Any Order

Abstract. A method is presented for obtaining from kinetic experiments both order of reaction and rate constant by means of a single straight-line graph, in contrast to previous methods, which require several steps including more than one graph or repeated trial-and-error calculations whenever there is no prior knowledge of the order of reaction.

Commonly used methods for evaluating reaction rate constants from experimental data either presuppose a knowledge of the reaction order or proceed by assuming an order in trial-and-error fashion. If, as frequently happens with complex and fractional-order reactions, the correct assumption is not made on the first trial, the computations can become tedious. This is true also of the direct determination of orders by the differential method of van't Hoff, which requires at least two separate plots as well as measurements of slopes often difficult to obtain with precision. Other direct methods, utilizing half-life periods or initial velocities for a series of reactant concentrations, are dependent on the availability of these additional experimental data for different initial concentrations.

All these procedures share the shortcoming of requiring a combination of several steps or plots before both order and rate constants can be evaluated. If the requisite number of trials is not made, the lack of sensitivity with respect to order inherent in some of these methods (for instance, the same set of data may give reasonably straight appearing lines if plotted according to the equations for more than one reaction order) can be the cause of inaccurate or misleading statements of the "order" of a reaction (see 1).

These considerations make it desirable to find a way of obtaining both kinetic constants in a single step. Referring to the general differential equation for a simple reaction

$$dx/dt = k(a - x) \quad (1)$$

(where a is the initial concentration; x is the amount reacted, in the same concentration units as a ; t is the elapsed time; k is the rate constant; and n is the order of reaction), it does not appear unreasonable to search for a straight-line plot of some suitable simple functions of the reaction variables such that the two parameters of the line (slope and intercept) would uniquely determine the two constants

(n and k) of the differential equation.

Through a heuristic approach guided by somewhat analogous relationships in other branches of physical science, a graphical treatment of data has been devised which essentially accomplishes this end (2). It is convenient here to follow the reaction in terms of the fractional extent of reaction, $f = x/a$, and to introduce a new variable termed "unit reaction time," defined as the ratio t/f . By appropriate algebraic manipulation of the integrated form of Eq. 1, using series expansions, one can express unit reaction time as a series in t ,

$$\frac{t}{f} = \frac{1}{a^{n-1}k} + \frac{n}{2} t + \frac{n(2-n)}{12} a^{n-1} k t^2 + \dots \quad (2)$$

It is seen that for $n = 2$ a plot of unit reaction time versus elapsed time is a straight line; for $n \neq 2$, the plot is found not to deviate greatly from a straight line at moderate values of f (for example, $f = 0.25$). Extrapolation toward $t = 0$ usually is a sufficient approximation to the tangent at the

ordinate intercept and yields the information of interest: the reaction order is obtained directly

$$\text{Initial slope} = n/2, \quad (3)$$

and k (with n and a known) is obtained from

$$\text{Ordinate intercept} = 1/(ka^{n-1}). \quad (4)$$

A longer paper containing the derivation of these relationships, together with a discussion of the limitations to which this treatment is subject, is in preparation. It may be mentioned here that, by its nature, the method is particularly sensitive to the early points of the reaction. Thus it is relatively little affected by the occurrence of consecutive or opposing reactions; however, timing errors due to uncertainty of the start of the reaction may introduce considerable curvature into the beginning of the plot. With suitable modifications the method is then still usable provided the curve straightens out sufficiently before the reaction is affected appreciably by other complications.

cating factors, such as side reactions. In unfavorable cases, additional kinetic data for different initial concentrations may be needed for an unambiguous determination of the order. Such eventualities are no reflection on the method, inasmuch as defects in the data would affect any other method also, though without always giving as clear an indication of their presence.

The method can be made applicable also to certain complex reactions and to reactions between reactants at unequal initial concentrations.

Plots (A) and (B) of Fig. 1 provide an example of the treatment of an identical set of data by a conventional method and by the present method, respectively (3). In plot (A), $n = 2$ has been assumed in plotting $1/(a - x)$ against t , to obtain a value of $k = 11.74 \text{ lit.} \cdot \text{mole}^{-1} \cdot \text{min}^{-1}$. (B), the plot of t/f against t , has a slope of 0.994, indicating $n = 2$, by Eq. 3; from the intercept, 8.64 min, and $a = 0.100 \text{ mole/lit.}$, k is then found to be $11.58 \text{ lit.} \cdot \text{mole}^{-1} \cdot \text{min}^{-1}$, by Eq. 4. Because different ways of plotting are mathematically equivalent to differing weighting of the experimental points, results in other cases may be in less close agreement, but at least a fair approximation for k can usually be expected from this simple and rapid method.

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2. In investigating this subject, I have become indebted to Robert Blum and to Gail L. Miller for valuable comments, to Warren Watanabe for communicating an independent and somewhat divergent development of substantially the same topic, and to Serge Gratch for a generous contribution of helpful discussion. The topic first came up for attention in late 1951 and was made the subject of a seminar presentation by me at Temple University on 14 May 1952. Mimeographed abstract copies were distributed at the time and have been on record since then at the chemistry department library. Journal publication, however, was delayed, because it was felt that it should await a thorough exploration of the limitations and modifications referred to, for which time and opportunity were then lacking. Recent revival of interest in the method on the part of a number of workers in these laboratories has prompted the conviction that in view of its potential utility publication should be delayed no longer.
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3 April 1961

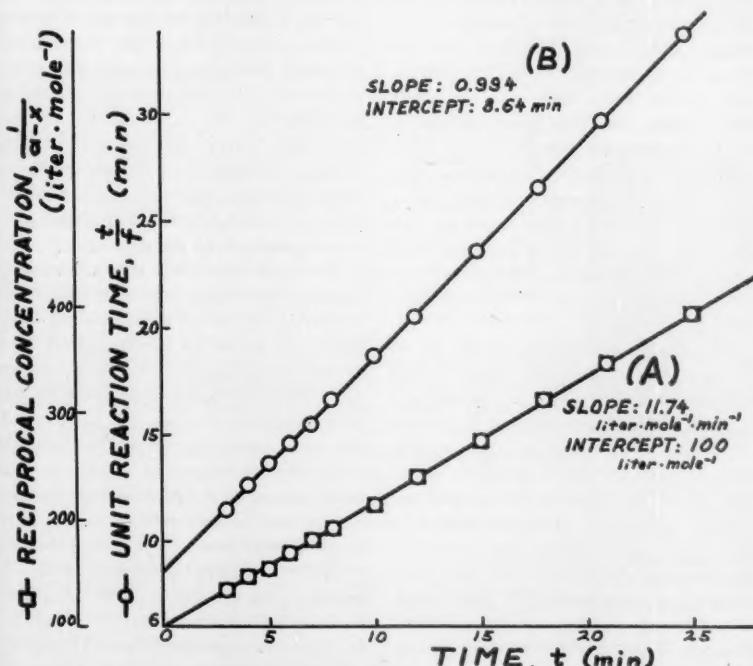


Fig. 1. Kinetics of saponification of methyl iodide by sodium hydroxide, at equal initial concentrations of 0.0100 mole/lit. Plot (A), conventional method, according to integrated form of second-order equation. Plot (B), "unit reaction time" method, without prior knowledge of order.

Meetings

Carbon Conference

Professors who like to challenge their students with novel parameters could do worse than call for a report on a world without carbon. From the thermonuclear fire that lights the sun to the cave fire of emergent man and into every plant and animal cell, carbon is woven inextricably into the fabric of life. And if rockets and reactors seem to have supplanted carbon as a fuel source, one still finds carbon valuable and perhaps essential to contain or control these energy sources. Fuel cells, thermoelectric devices, and other electronic developments add to the glamor of carbon. Thus this oldest of materials takes on new life in an age that moves inexorably through the performance thresholds of time-tested materials.

The new role of carbon can be measured in the brief history of a group organized by S. Mrozowski, of the University of Buffalo, as the American Carbon Committee. The group called a somewhat impromptu conference in 1953, and 80 persons heard the four papers that constituted the entire proceedings. Yet the need for such a conference was so clear that it was continued, and in 1955, 18 papers were presented to an audience of 220. For the third biennial meeting in 1957, there were 80 papers and 305 participants; and in 1959, 86 papers were given for 335 persons. The fifth biennial Carbon Conference in 1961, the first meeting to be held away from Buffalo, drew 135 papers and 430 participants—including 77 from other nations—to Pennsylvania State University on 19–23 June.

This exponential growth has its problems in conference logistics. The American Carbon Committee has some money problems as the proceedings grow larger and larger (Pergamon Press will publish the 1961 proceedings in two volumes). The size of the conference, as well as the broadening interest

of participants, requires concurrent sessions and a departure from the give-and-take that characterizes the small meeting. Yet the general quality of the papers remains so high and the interest so great that no thought was given to terminating the biennial conference; the second decade of the conference will be inaugurated at Pittsburgh in June 1963.

Perhaps the outstanding feature of the 1961 conference was the large number of papers from overseas, signifying that carbon research has reached a relatively advanced state throughout the world. The United Kingdom led the overseas contingent with 20 papers; nine papers came from France, seven from Japan, and four from Australia. There were papers also from West Germany, Belgium, Italy, Switzerland, and India, but an eagerly awaited paper from the U.S.S.R. fell victim to visa problems. Of the domestic papers, 40 were from industrial laboratories, 24 from universities, and 19 from governmental laboratories.

Knowledge of carbon stands at an interesting position. For years, it fell between the desks of the metallurgist and the ceramist, not meeting the classical definition of either, and the organic chemist gathered most of the information about this material. But for the last decade, physicists, chemists, metallurgists, fuel technologists, and ceramists have been making up for lost time, and carbon is by now one of the best-known materials. Notwithstanding, questions from the floor of the conference more often than not were met with the response "We don't know," or "We hope to go into that soon." This response in reality represents a strength rather than an apparent weakness, for it means that, so to speak, carbon workers are really getting to the heart of the matter. Materials scientists, led by carbon scientists, appear to stand near the position of nuclear physicists in the early 1930's. The stimulus, of the space and nuclear

programs and the new electronics are the catalytic forces driving materials science to a new plateau, and carbon scientists appear to be leading the way. Certainly, the interdisciplinary nature of much carbon research and the new measurement and examination techniques can be applied to materials generally.

After a general introductory session of three papers from abroad on electronic properties, diamond-oxygen reaction kinetics, and the surface properties of extremely thin graphitic lamellae, the conference broke up into concurrent sessions for groups of papers on graphitization, pyrolytic graphites, surface chemistry of carbon blacks, dislocations and high temperature properties, electron spin resonance, radiation effects, reactivity of graphite, electronic properties, uses and applications, surface chemistry of porous carbons, mechanical properties, structural studies, and kinetic and surface reactions.

Persons who had attended previous conferences found the most significant advances reported in the papers on pyrolytic graphite and new electron-microscopy techniques to study graphitic structure. The new pyrolytic graphites, produced by the decomposition of hydrocarbon gases, provide materials of unusual strength, purity, and chemical properties that hold out good possibilities for meeting some of the rigid materials demands of the day.

To some attending this conference, detailed knowledge of the structure and properties of carbon on the atomic scale appeared to be on the threshold of a major breakthrough, which will become possible through the availability of sizable synthetic crystals of graphite. A thorough understanding of the electrical and physical properties of carbon has been held up in the past because the material could not be studied in the pure single-crystal form. The properties of graphite crystallites are remarkably different along the different crystal directions and cannot be determined uniquely from measurements upon aggregates of small crystals or powders. The melting point of carbon is so high that to obtain sizable crystals of either graphite or diamond has baffled scientists in the past. With the availability of new equipment and techniques for attaining ever higher temperatures and pressures, however, it looks as if man soon will be able to produce good crystals of both graphite and diamond.

G. R. Hennig (Argonne National

Kodak reports on:

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A phantom



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such, its *Tenite* thyroid, liver, lungs, spleen, kidneys, and other organs filled with radioactive solutions that simulate the radiative properties of a radioisotope-treated organ. Alternatively the *Tenite* shell can be equipped with a human skeleton inside and a system of dosimeter ports for studies of absorption of radioactivity.

How the mind wanders

Methanesulfonyl Fluoride (Eastman 8368) is the result of a day spent listening in on the ACS Division of Medicinal Chemistry and in particular a paper on "Preparation of Some Purinesulfonamides from the Corresponding Purinesulfonylfluorides." We don't recall its content with any precision. What the speaker succeeded in doing to us was to impress us with the strong role being played in biochemical thought today by the sulfonyl group. His mind was doubtless illuminating the big picture, but our mind was narrowing down to the little picture. Here was a trend, and we ought to make sure we were riding it well.

We offer *Methanesulfonic Acid*, *Methanesulfonic Anhydride*, *n-Butyl Methanesulfonate*, *Ethyl Methanesulfonate*, *Methyl Methanesulfonate*, and *Methanesulfonyl Chloride*. This fellow, we noted, was talking sulfonylfluoride. We have a man who is skilled in the art of exchanging any halogen for any other. We decided that as soon as we got home we would have him switch over some chloride to fluoride. That he has done. We are now ready to accept our small but rightful reward for thoughtfulness.

If you want some, or, what is more likely, a look at the list of some 3900 Eastman Organic Chemicals which we stock and which we properly reveal in our Catalog No. 42, get in touch with: Distillation Products Industries, Rochester 3, N. Y. (Division of Eastman Kodak Company).

A little x-ray news

More precious than rubies is confidence in the importance of what one does for a living. One thing we do for a living is to manufacture x-ray film. Unkind words are rarely spoken about society's need for x-ray film. Now we have

news about x-ray film and need to make it seem important. Easy.

The first piece of news has it that *Kodak Industrial X-ray Film, Type M* is now obtainable with emulsion on one side only instead of both sides, the way x-ray film usually comes in order to double the strength of the image. Simple, yes; trivial, no. Ties in to the very large subject of mankind's current push for great structural strength in small mass. Load-bearing members are now getting so thin that putative flaws on their radiographs have to be checked out with a microscope. Since a microscope can focus on only one side of the film at a time, it's better to have the other side blank. Enough of this is being done now so that x-ray dealers are stocking the single-coated film of high contrast and fine grain.

Eastman Kodak Company, X-ray Division, Rochester 4, N. Y., will be glad to guide you to such a dealer.

The second piece of news much exceeds the first in importance. The nuclear testing debate has gone on for years. As an intelligent citizen, you have been given estimates by various authorities of how much radiation you and your children can expect to soak up, barring disaster. You have been told how much to figure for medical and dental radiological examination over a lifetime. Meanwhile we have been quietly goofing up the statistics! We have been upping the response of the films. With the latest step, the same amount of examination requires half or a third as much radiation as had been estimated.

No action is required on your part. Just privately rejoice a little at how the deal has been sweetened a bit for you, statistically.

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This is a professional motion picture camera. It is called the *Kodak Reflex Special Camera (16mm)*. Its price is \$1895. (Any amateur who buys it imperils his amateur status, but that's his lookout, not ours.) For a professional camera the price is remarkably low, particularly since it is the latest and most versatile of professional 16mm cameras and everybody knows that the cost of professional

equipment in most fields rarely goes in any other direction than up. The base price includes a removable synchronous motor for 24 frames/sec, both 400- and 100-foot film chambers, and a Kodak Cine Ekton Lens, 25mm f/1.4. An accessory is available for any kind of time-lapse photography. Another accessory records synchronized sound. Etc., etc.

However long this recital were spun out, we doubt that the person for whom we spent 10 years making this camera is the kind who would commit himself on the strength of this ad. If he will signify his interest to *Eastman Kodak Company, Motion Picture Film Department, Rochester 4, N. Y.*, we shall work out some arrangement to bring him, the camera, and its accessories together.

Price subject to change without notice.

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Laboratory) served as program chairman for the 1961 meeting. The local chairman was P. L. Walker, Jr. (Pennsylvania State University), who with S. Mrozowski and M. L. Studebaker (Phillips Chemical Company) is a member of the executive committee of the American Carbon Committee. David S. Coleman (Pennsylvania State University) served as conference coordinator.

M. E. BELL

Pennsylvania State University,
University Park

Forthcoming Events

October

1-3. Council for Intern. Organizations of Medical Sciences, Paris, France. (CIOMS, 6 rue Franklin, Paris 16)

1-4. Process Engineers, annual, Vienna, Austria. (Oesterreichischer Ingenieur- und Architektenverein, Eschenbachgasse 9, Vienna 1)

1-5. Electrochemical Soc., Detroit, Mich. (Electrochemical Soc., 1860 Broadway, New York 23)

1-7. International Special Committee on Radio Interference, plenary session, Philadelphia, Pa. (S. D. Hoffman, American Standards Assoc., 10 E. 40 St., New York 16)

1-8. International Congr. of Industrial Chemistry, 33rd, Bordeaux, France. (Société de Chimie Industrielle, 28 rue Saint-Dominique, Paris 7, France)

2-4. Communications Symp., 7th natl., Utica, N.Y. (R. K. Walker, 34 Bolton Rd., New Hartford, N.Y.)

2-7. Climatic Change, symp., Rome, Italy. (UNESCO, Place de Fontenoy, Paris 7, France)

2-7. International Astronautical Federation, 12th congr., Washington, D.C. (American Rocket Soc., 500 Fifth Ave., New York 36)

2-7. Inter-Regional Leprosy Conf., Istanbul, Turkey. (WHO, Regional Office for Europe and Regional Office for the Eastern Mediterranean, 8 Scherfigsvej, Copenhagen Ø, Denmark)

2-11. International Council for the Exploration of the Sea, 49th annual, Copenhagen, Denmark. (Charlottenlund Slot, Charlottenlund, Denmark)

3-5. Physics and Nondestructive Testing, symp., Argonne, Ill. (W. J. McGonagle, Argonne Natl. Laboratory, 9700 S. Cass Ave., Argonne)

3-8. Aerosol Congr., 3rd intern., Lucerne, Switzerland. (Federation of European Aerosol Assoc., Waisenhausstrasse 2, Zurich, Switzerland)

4-10. Latin American Congr. of Electroencephalography, 5th, Mexico, D.F. (J. Hernandez Paniche, Instituto Mexicano de Seguro Social, Hospital La Raza, Mexico, D.F.)

4-10. Latin American Congr. of Neurosurgery, 9th, Mexico, D.F. (J. H. Mateos, Tonalá No. 15, Mexico 7, D.F.)

6-7. American Medical Writers' Assoc., New York, N.Y. (S. O. Waife, P.O. Box 1796, Indianapolis 6, Ind.)

6-8. Therapeutics, 7th intern. congr., Geneva, Switzerland. (P. Rentchnick, Case Postale 229, Geneva 2)

8-10. Zooplankton Production, symp., Copenhagen, Denmark. (J. H. Frazer, Marine Laboratory, P.O. Box 101, Victoria Rd., Aberdeen, Scotland)

8-11. Society of American Foresters, Minneapolis, Minn. (H. Clepper, SAF, 425 Mills Bldg., Washington 6)

8-13. American Acad. of Ophthalmology and Otolaryngology, Chicago, Ill. (W. L. Benedict, 15 Second St., SW, Rochester, Minn.)

9-11. National Electronics Conference and Exhibition, 17th annual, Chicago, Ill. (NEC, 228 North La Salle St., Chicago 1, Ill.)

9-12. Instrument Symp. and Research Equipment Exhibit, 11th annual, Bethesda, Md. (J. B. Davis, Natl. Institutes of Health, Bethesda 14)

9-12. Water Pollution Control Federation, 34th annual, Milwaukee, Wis. (R. E. Fuhrman, 4435 Wisconsin Ave., NW, Washington 16)

9-13. American Rocket Soc., space flight meeting, New York, N.Y. (ARS, 500 Fifth Ave., New York 36)

9-13. Luminescence of Inorganic and Organic Systems, intern. conf., New York, N.Y. (Miss G. M. Spruch, New York Univ., Washington Sq., New York 3)

10-12. Nuclear Reactor Chemistry, 2nd conf., and Analytical Chemistry in Nu-



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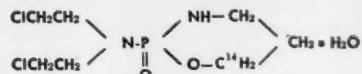
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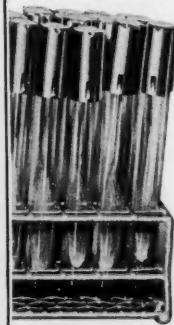
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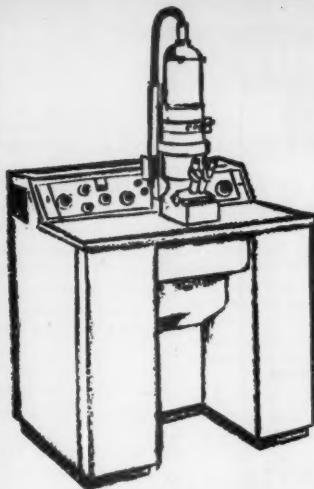
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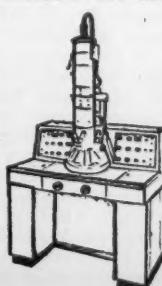
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clear Reactor Technology, 5th conf., Gatlinburg, Tenn. (Oak Ridge National Laboratory, Post Office Box X, Oak Ridge, Tenn.)

10-13. Administration of Research, 15th conf., San Juan, Puerto Rico. (G. F. Anton, Research Center, Univ. of Puerto Rico, Mayaguez, P.R.)

10-20. International Committee for Biological Control, Tunis. (P. Grison, Laboratoire de Biocenotique et de Lutte Biologique, La Miniere, par Versailles (S.-et.-O.), France)

11-13. Gaseous Electronics Conf., American Physical Soc., Schenectady, N.Y. (C. J. Gallagher, General Electric Research Laboratories, Schenectady, N.Y.)

11-14. Tau Beta Pi Assoc., Cincinnati, Ohio. (R. H. Nagel, Univ. of Tennessee, Knoxville)

11-14. Western Inst. on Epilepsy, 13th annual conf., San Antonio, Tex. (F. Risch, 3097 Manning Ave., Los Angeles, Calif.)

12-13. Congress of Neurological Surgeons, New York, N.Y. (E. Weiford, 4706 Broadway, Kansas City 12, Mo.)

12-29. Pacific Intern. Trade Fair, 2nd, technical meetings, Lima, Peru. (PITF, P.O. Box 4900, Lima)

14-20. International Congr. of Neurological Surgery, 2nd, Washington, D.C. (B. S. Ray, 525 E. 68 St., New York 21)

15. American College of Dentists, Philadelphia, Pa. (O. W. Brandhorst, 4236 Lindell Blvd., St. Louis, Mo.)

15-20. American Inst. of Electrical Engineers, fall general meeting, Detroit, Mich. (E. C. Day, AIEE, 33 W. 39 St., New York 18)

15-20. International Congr. of Allergology, 4th, New York, N.Y. (W. B. Sherman, 60 E. 58 St., New York 22)

15-21. Pan American Congr. of Endocrinology, 5th, Lima, Peru. (M. San Martin, Av. Central 325, San Isidoro, Lima)

16-17. Engineering Writing and Speech, natl. symp., East Lansing, Mich. (J. D. Chapline, Philco Corp., 3900 Welsh Rd., Willow Grove, Pa.)

16-17. Ionization of the Air, intern. conf., Philadelphia, Pa. (I. C. Kornbluh, American Inst. of Medical Climatology, 1618 Allengrove St., Philadelphia 24)

16-18. American Soc. of Safety Engineers, Chicago, Ill. (A. C. Blackman, 5 N. Wabash Ave., Chicago 2)

16-18. Entomological Soc. of Canada and Entomological Soc. of Quebec, Quebec, Canada. (L. L. Reed, ESC, Neatby Bldg., Carling Ave., Ottawa, Canada)

16-18. Metallurgy of Beryllium, intern. conf., London, England. (Secretary, Inst. of Metals, 17 Belgrave Sq., London, S.W.1)

16-19. American Dental Assoc., Philadelphia, Pa. (H. Hillenbrand, 222 E. Superior St., Chicago 11, Ill.)

16-19. Vacuum Science and Technology, 2nd intern. congr., Washington, D.C. (W. M. Welch, Intern. Organization for Vacuum Science and Technology, 1515 Sedgwick St., Chicago 10, Ill.)

16-20. American Ornithologists' Union, Washington, D.C. (H. G. Deignan, U.S. National Museum, Washington 25)

16-20. American Soc. of Civil Engineers, New York, N.Y. (W. H. Wisely, 33 W. 39 St., New York 18)

(See issue of 18 August for comprehensive list)

New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable. Neither Science nor the writer assumes responsibility for the accuracy of the information. All inquiries concerning items listed should be addressed to the manufacturer. Include the department number in your inquiry.

Time-code generator produces a serial readout of 20-bit, 24-hour code. Frequency stability is said to be 3 parts in 10^8 per day. The instrument also furnishes a 1-Mcy/sec differentiated square wave for synchronization with external standards and a 1-pulse-per-second output for comparison with time ticks from radio station WWV. The unit is designed for operation over an ambient temperature range of -20° to $+55^{\circ}$ C. An external signal may be substituted for the internal crystal oscillator. (Electronic Engineering Co., Dept. Sci295, 1601 E. Chestnut Ave., Santa Ana, Calif.)

Automatic monitor has a scanning rate of up to 5000 inputs per second and measures any variable reducible to a voltage signal as well as the rate of change of the variable. The monitor compares measured values with preset values and converts the result into digital form. Alarm conditions are displayed by a cathode-ray tube. Five types of alarm conditions are recognized: high alarm; excessive rate of change; approach to trip; irrational high; and irrational low. The system can be expanded to 3600 input channels. Systems with self-healing as well as self-checking features are available. (Monitor Systems, Inc., Dept. Sci296, Fort Washington Industrial Park, Fort Washington, Pa.)

Digital readout device decodes either binary inputs or pulse trains and displays information in digital form. Sixteen $3\frac{1}{4}$ - by $1\frac{1}{4}$ -in. alphanumeric characters are displayed at rates up to 50 per second. Ten of the symbols are decimal digits; the other six can be specified by the customer. A built-in memory keeps the last character in position with no power requirement. Operation is on either 12 or 28 volts d-c with power requirement 0.57 watt sec per bit. Size is 1 by 3.1 by 2.5 in. (Datascope Corp., Dept. Sci330, Culver City, Calif.)

Cardiovascular measurement equipment offers two to four simultaneous intercardiac pressures for left and right heart catheterization. The equipment includes a four- to eight-channel oscil-

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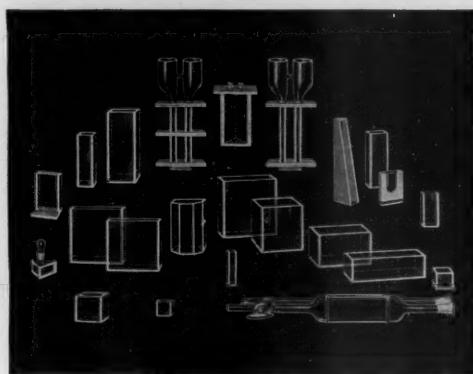
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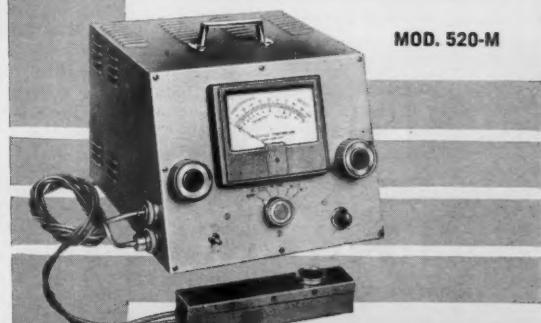
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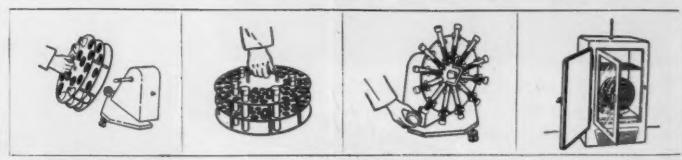
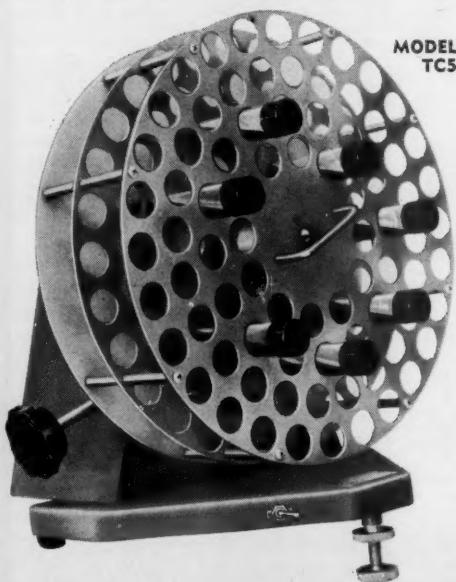


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OSCILLOSCOPE camera, which uses the Polaroid-Land camera principle, allows up to nine trace exposures per print. Full-size prints may also be obtained. The camera uses a $f/1.9$ Wollensak Oscillo-Raptar 75-mm coated anastigmatic lens. (Electronic Tube Corp., Dept. Sci309, 1200 E. Mermaid Lane, Philadelphia 18, Pa.)

Temperature controller uses as sensor a 500-ohm platinum-resistance transducer. Output is a set of relay contact closures that can be set from 2 to 15 amp at 115 volts a-c. Range of the instrument is 0° to 2000°F. Temperature differential between on and off is said to be less than 2.5°F over the entire range. A nulling meter indicates deviation from set point in degrees Fahrenheit; resolution is $\pm 0.5^\circ\text{F}$. (Winsco Instruments and Controls Co., Dept. Sci281, 11789 W. Pico Blvd., Los Angeles 64, Calif.)

Impedance bridge measures capacitance and dissipation factor of capacitors, inductance and storage factor of inductors, and a-c and d-c resistance of resistors. Resistance is measured from 0 to 12 megohms in eight ranges with accuracy said to be $\pm(0.1\text{ percent} + 1\text{ div.})$. Capacitance is measured in seven ranges from 0 to 1200 μf with accuracy of $\pm(0.2\text{ percent} + 1\text{ div.})$. Inductance range is 0 to 1200 h in seven steps with accuracy of $\pm(0.3\text{ percent} + 1\text{ div.})$. Dissipation factor and storage factor are measured from 0 to 1000 at 1 kcy/sec with accuracy $\pm(2\text{ percent} + 0.005)$. (John Fluke Manufacturing Co., Dept. Sci311, Mountlake Ter., Wash.)

Phase generator-shifter consists of a tuning-fork oscillator, filter circuit, resistance-capacitance phase shifting networks, an electron-tube phase inverter, and two cathode followers. Phase angle is continuously variable from 0 to 360 deg. For direct phase reading, frequency is normally 400 or 800 cy/sec. Other frequencies from 50 to 1000 cy/sec. are available. Accuracy is said to be ± 0.1 deg relative and 0.25 deg absolute. Impedances looking into the output terminals are 300 ohms nominal shunting resistance and 2 μf series capacitance for d-c blocking. Variation of load impedance is said to have no effect on accuracy of phase shift. (Ad-Yu Electronics Lab., Inc., Dept. Sci344, 249-259 Terhune Ave., Passaic, N.J.)

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Ph.D. or equivalent. Research institute, New York. Must have training and experience in all phases of preparation of tissues and in handling RCA EMU3.

Box 182, SCIENCE

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Ph.D. or equivalent—quantitative enzymatic microhistochemistry of brain and/or pancreas. Research institute, New York.

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To join research team on Candidiasis; hospital in Brooklyn, N.Y. Salary depending on training and experience. Send résumé to: Box HA-432, 56 West 45 Street, New York 36, N.Y.

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NEW ZEALAND SENIOR RESEARCH FELLOWSHIPS. Applications are invited for Senior Research Fellowships to be taken up in New Zealand in 1962. Applicants should possess a Ph.D. degree, or an equivalent qualification, or while not having this qualification have a distinguished record of scientific research. There is no age limit and no restriction regarding nationality, but successful applicants must meet New Zealand immigration requirements. A good working knowledge of English is required.

The fellowships will be awarded for research at any of the branches of the New Zealand Department of Scientific and Industrial Research. They will be tenable for periods of between 6 months and 1 year initially, but may be extended to a total period of 2 years. The value of each fellowship will range between £1400 and £1900 a year, with an additional £250 a year for a married man. These amounts are taxable. (NZ\$1 equals \$2.80 or £1 sterling.) A grant for return fares by surface transport to New Zealand at tourist class rates is payable to each fellow, and an allowance of two-thirds of this grant may be paid where a fellow is accompanied by his wife. While in New Zealand reasonable traveling expenses incurred in connection with the fellow's research work will be paid.

Application forms and further particulars are obtainable from:

1) The New Zealand Scientific Liaison Office B.C.S.O., Africa House, Kingsway, London W.C.2, England.

2) The New Zealand Embassy, 19 Observatory Circle, Washington 6, D.C., U.S.A.

3) The Office of the High Commissioner for New Zealand, Suite 804, Commonwealth Buildings, 77 Metcalf Street, Ottawa, Ontario, Canada.

4) The Department of Scientific and Industrial Research, P.O. Box 8018, Wellington, New Zealand.

Applications should reach New Zealand by 30 December 1961.

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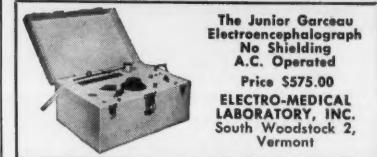
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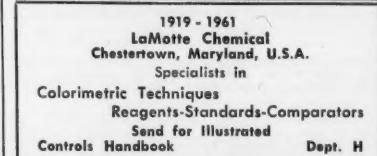
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APPLICATION FOR HOTEL RESERVATIONS

128th AAAS MEETING

Denver, 26-31 December 1961

The hotels for the AAAS Denver meeting have established special, low rates and have reserved appropriately large blocks of rooms for this meeting. Thus everyone making room reservations for the AAAS meeting is assured substantial savings.

The list of hotels and the reservation coupons below are for your convenience in making your hotel reservation in Denver. Please send your application, *not* to any hotel directly, but to the AAAS Housing Bureau in Denver and thereby avoid delay and confusion. The experienced Housing Bureau will make assignments promptly; a confirmation will be sent you in two weeks or less.

If requested, the hotels will add a comfortable rollaway bed to any room, at \$3.00 per night. Mail your application now to secure your first choice of desired accommodations. All requests for reservations must give a definite date and estimated hour of arrival, and also probable date of departure.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

For a list of the headquarters of each participating society and section, see page 197, *Science*, 21 July. The Hilton is the AAAS headquarters hotel.

Rates for Rooms with Bath*

Hotel	Single for one	Double for one	Double for two	Twin Beds for one	Twin Beds for two	Studio Twins	Suites
Hilton	\$8.50	\$10.00	\$14.00		\$14.00	\$15.00	\$27.00 to \$55.50
Brown Palace	8.00	9.00	13.00	\$10.00	15.00		24.00 to 65.00
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* All rooms are subject to a 2% Colorado State sales tax.

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AAAS Housing Bureau
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Date of Application

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Number in party Sharing this room will be:

(Attach list if this space is insufficient. The name and address of each person, including yourself, must be listed.)

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